

25

New Energy Times Archive

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Date: 25 January 1990, 17:20:39 EST

From: (R.L.Garwin (914) 945-2555) RLG2 at YKTMV
IBM Fellow and Science Advisor to the Director of Research
P.O. Box 218
Yorktown Hts, NY 10598
To: GAI at YALEVM
JONESSE at BYUVAX
KGL at APSEDOFF

Subject: Conclusions.

Reply-To: RLG2 at WATSON

Right now I find no significant disagreement between the efficiencies calculated by Al Anderson, myself, and Moshe Gai for 2 or more votes among the 10 ring counters, or for 4 or more votes.

Actually, from my tables, efficiency for 2 or more votes among the ring counters (with $M=40$ neutrons in a burst) would be 80.55% rather than the "at least 90%" cited by Gai at the foot of page 11 (Draft 2, received by me 01/23/90). Al Anderson quotes 90.45% for 50, 45.69% for 20 neutrons; my table shows 89.16% for 50 neutrons-- hardly a significant disagreement.

So if people can avoid arguing about who said what when, and who said what first, they can get down to the significance of the results. Naturally, there should be a reasonable derivation of efficiency in the paper; I offer the view that all these numbers are small enough that there is no need for power-series expansions that raise a red flag of "probabilities exceeding unity." I would think that the paper should quote some of the Monte Carlo results of Anderson in comparison with analytical results.

What does it mean? The Los Alamos results are not made more significant by including long-duration counting, if the bursts always come early in the run; so the comparison should be made with the maximum rate observed at LANL at the early parts of the run-- compare with burst RATE rather than with total bursts divided by total time. The Menlove, Garcia, Jones paper LAUR 89-3633 (10/16-18/89) states (Page 5)

"... the majority of neutron bursts occur during warmup from LN; however the time period while the sample is between -100 C and 0 C represents less than 5% of the sample counting time."" and

"It is highly significant that ALL of the high-yield bursts occurred during the LN warm-up at -30 C."

From Table II one can see 19 bursts with neutron numbers greater than 50-- many MUCH greater than 50, for which the ring counter should give 2 or more votes with probability exceeding 90%. Taking into account the 50% probability of 2 or more votes with neutron numbers as small as 22, one can simply say that no neutron bursts exceeding 40 occurred within the apparatus at Yale during the running time of the experiment. And one could go on to say that if the conditions were the same as at LANL, some xx events exceeding $M = 40$ would have been expected.

Of course, if only 2 events were anticipated, even identical source conditions would have detected the anticipated rate with an efficiency

1-e**-2

since there would be a probability e^{*-2} that none of the two expected events occur. This small-number probability is much more important than the inefficiency of the detector.

Of course, if the conditions can be created only at LANL, Yale doesn't have a chance... But that's what publishing experimental results is all about.

Dick Garwin

New Energy Times Archive

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Received: by YALEVM (Mailer R2.03B) id 6147; Thu, 25 Jan 90 10:50:27 EST
Date: Thu, 25 Jan 90 10:29:07 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI@YALEVM>
Subject: Hit Probability
To: "R.L. Garwin" <rlg2@watson>,
Steve Jones <jonesse@byuvax>,
kelvin lynn <kg1@apsedoff>

Dear Dick,

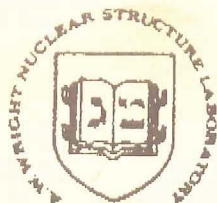
Thank you for sending to me your solution for hit probabilities. I must add that I have enjoyed reading your letter. Your way of calculating hit probability is different than mine and I always enjoy seeing things from a different angle. I particularly enjoyed learning of your trick for calculating no hit (no vote) from a counter, which I have now derived using a Poisson distribution. Please note that your equation at the bottom of page 2 is missing an e, seems like a typo.

Your hit probability P2RP, for 2 or more hits in ten ring counters independently of the two central detectors (A and B), is directly related to the sum of curves for $K=2, 3, 4$, etc. of my Fig. 5 of draft number 2. I FAXed to you this figure a few days back. I will FAX to you the comparison of the two solutions. The two solutions appears almost indistinguishable. The largest disagreement occurs for multiplicity $M=40$ where you calculate 80.55% probability and I calculate 81.61% probability.

Let me than thank you for taking the time to calculate these probability, and for bringing to my attention a different way of solving the problem. I hope you agree that my solution as outlined in draft number 2, equation (2), is in fact equivalent to your solution.

I am still puzzled by the question of the best way quote an upper limit on the burst size, having calculated hit probabilities, and not observing a particular fold and higher. Yesterday you told me on the phone that my solution, as outlined in draft 2, could be improved to yield a tighter upper bound. I am awaiting your solution.

Thank you again Moshe Gai.



YALE UNIVERSITY

A. W. WRIGHT NUCLEAR STRUCTURE LABORATORY

P.O. Box 6666, 272 Whitney Avenue, New Haven, Connecticut 06511

DATE

1/25/90

NUMBER OF PAGES INCLUDING THIS COVER SHEET:

3

THIS FAX IS TO:

Dick Garwin

FROM:

Moshe Gai

SPECIAL INSTRUCTIONS:

OUR FAX NUMBER IS: (203) 432-3522

For confirmation or questions, please call (203) 432-3090

25 JAN 90 2.48

-R.L. GARWIN-

P.2

Date: Thu, 25 Jan 90 10:29:07 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI@YALEVM>
Subject: Hit Probability
To: "R.L. Garwin" <rlg2@watson>,
Steve Jones <jonesse@byuvax>,
kelvin lynn <kgl@apsedoff>

39

Dear Dick,

Thank you for sending to me your solution for hit probabilities. I must add that I have enjoyed reading your letter. Your way of calculating hit probability is different than mine and I always enjoy seeing things from a different angle. I particularly enjoyed learning of your trick for caculating no hit (no vote) from a counter, which I have now derived using a Poisson distribution. Please note that your equation at the bottom of page 2 is missing an e, seems like a typo.

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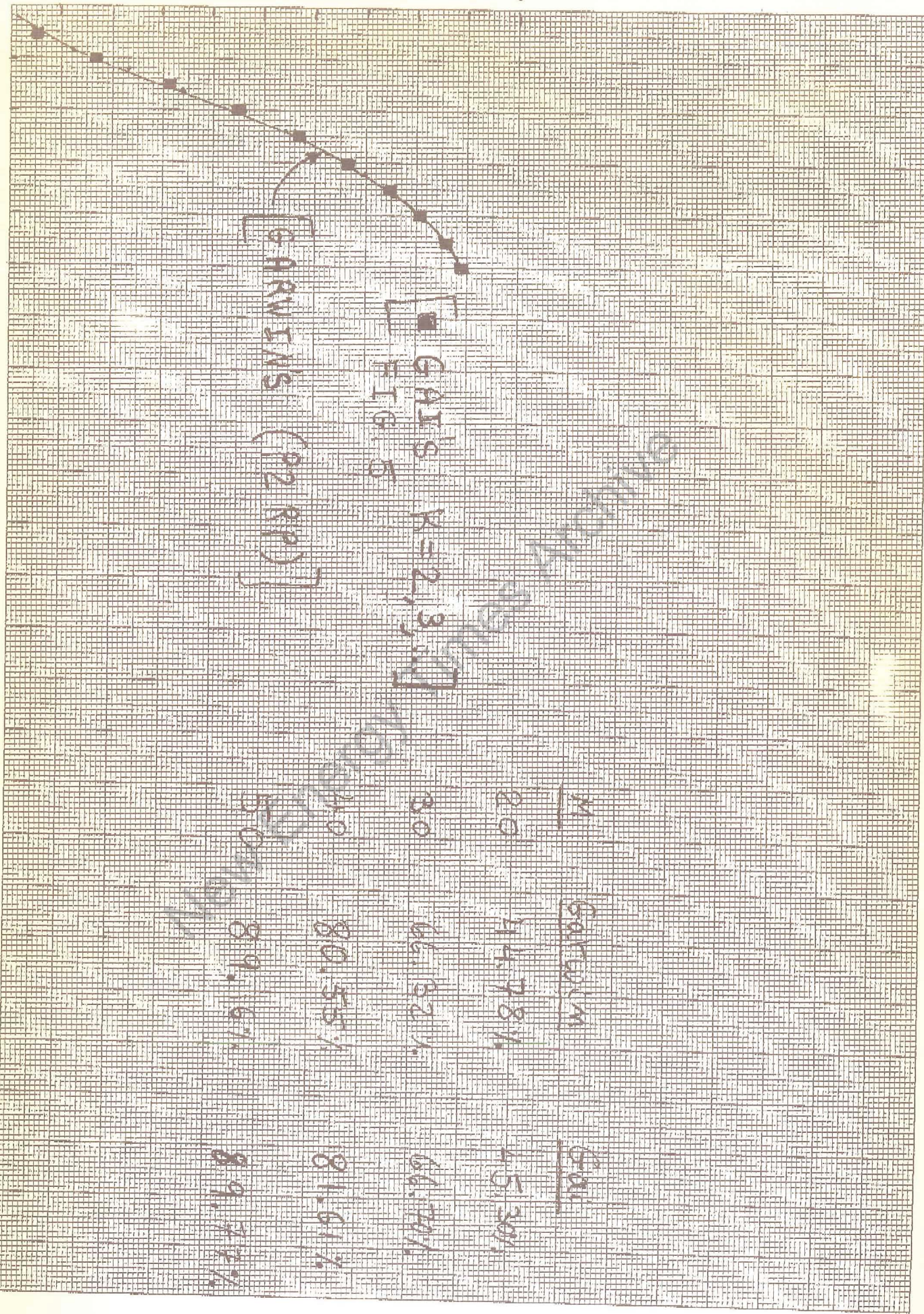
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Thank you again Moshe Gai.

MOSHE GAI, (203)432 R.L. Garwin

1/25/90 Hit Probability

^Z



46 1510

KE 10 X 10 TO THE CENTIMETER 18 X 25 CM. KEUFFEL & ESSER CO. MADE IN U.S.A.

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

January 24, 1990

Professor Moshe Gai
A.W. Wright Nuclear
Structure Laboratory
Yale University
New Haven, CT 06511

Dear Moshe,

Here is a hard-copy of what I have just sent you by BITNET and FAX.

My primary reason for writing, however, is to enclose the materials I received from Steve Jones. I hope that in the future you will be able to rely on Steve Jones to send you the materials!

I have now looked at Dr. Anderson's calculation, with which mine has a lot in common. I hope that he, too, will be motivated to find errors in my calculation.

Very best regards.

Sincerely yours,

Richard L. Garwin

Encl:

01/24/90 LTR RLG to M. Gai re results of calculation
outlined in RLG letter of 01/23/90.
-01/24/90 FAX RLG to M. Gai re source of tables.
+01/19/90 LTR S.E. Jones to RLG. (011990.SEJ)

cc:

S.E. Jones, BYU (Via FAX to 9 (801) 378-2800).

RLG:jah:PO24MG1:012490.MG1

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

January 24, 1990
(Via BITNET to GAI at YALEVM)

Professor Moshe Gai
A.W. Wright Nuclear
Structure Laboratory
Yale University
New Haven, CT 06511

Dear Moshe,

Here are the results of the program of calculations outlined in my letter of 01/23/90.

First, I assumed that for a single neutron liberated into the assemblage, there is a probability of 0.10 of giving a count in counter A, 0.10 of giving a count in counter B, and 0.008 of giving a count in each of ten other counters. To be precise, I assumed that each of these probabilities was independent of any other, so that the overall probability of detection of a single neutron is precisely 0.28. At the end, I shall show how to compensate to provide an approximate result on the alternative assumption that a neutron could count only in a single counter.

As an illustration, and in confidence that the experimenters will want to do their own calculations in full detail along these lines as well as in other ways, I have calculated the probability of having four or more counters each detect at least one neutron in a burst of M neutrons. Analogous results could, of course, be obtained for three or more counters, five or more counters, etc.

One can write a general expression for arbitrary numbers of counters of arbitrary individual efficiency, but because I did this in my head while driving the car, I treat counters A and B separately from the gang of ten. My program is first to calculate the probability of detection in both A and B, and in two or more of the gang of ten. I then calculate the probability of detection in A and not B, and in three or more of the gang of ten. There is an identical probability of being counted in B and missed in A. Then I calculate the probability of being counted in neither A nor B, but in four or more of the gang of ten.

With the specified assumptions, the probability of four or more counters registering when subject to a burst of

90/01/23 22:09:29 *** Function GAI ***

▽ GAI
 [1] $PA \leftarrow 1 - *(-M \times EPS1)$
 [2] $NS \leftarrow *(-M \times EPS2)$
 [3] $YS \leftarrow 1 - NS$
 [4] $P2 \leftarrow (PA \times 2) \times P2RP \leftarrow (1 - (NS \times N) \times (1 + N \times YS \div NS))$
 [5] $P1 \leftarrow PA \times (1 - PA) \times 1 - (NS \times N) \times TMP \leftarrow 1 + (N \times YS \div NS) + N \times (N - 1) \times (1 \div 2 \times 1) \times (YS \div NS) \times 2$
 [6] $P0 \leftarrow ((1 - PA) \times 2) \times 1 - (NS \times N) \times TMP + N \times (N - 1) \times (N - 2) \times (1 \div 3 \times 2 \times 1) \times (YS \div NS) \times 3$
 [7] $P \leftarrow P2 + P1 + P1 + P0$
 [8] $P4RP \leftarrow 1 - (NS \times N) \times TMP + N \times (N - 1) \times (N - 2) \times (1 \div 3 \times 2 \times 1) \times (YS \div NS) \times 3$
 [9] 'PROBABILITIES ARE NAMED AS FOLLOWS:'
 [10] 'P2 IS BOTH A AND B, AND 2 OR MORE OF 10 RING COUNTERS'
 [11] 'P1 IS A AND NOT B, AND 3 OR MORE OF 10 RING COUNTERS'
 [12] 'P0 IS NEITHER A NOR B, AND 4 OR MORE OF 10 RING COUNTERS'
 [13] 'P IS P2 + P1 + P1 + P0, SO 4 OR MORE COUNTERS OF ANY TYPE'
 [14] 'P2RP IS 2 OR MORE OF 10 RING COUNTERS, INDEPENDENT OF A AND B'
 [15] 'P4RP IS 4 OR MORE OF 10 RING COUNTERS, INDEPENDENT OF A AND B'
 ▽

Moshe & Steve,

Here is the source of
my tables.

Dick GARWIN

New Energy Times Archive

M neutrons is then obtained precisely as the sum of these four probabilities, which involve no "double counting."

I emphasize that an individual counter has only one vote in this operation, no matter how many neutrons may have collided in its active material.

The fundamental basis of this calculation is the recognition that with a given counter efficiency ϵ , and a number of neutrons in the assemblage M, the expected number of counts in that counter is $M\epsilon$. Of course, no matter how many "counts," there is only one vote. But the probability of no counts is then $e^{-(M\epsilon)}$. This is a result readily obtained from the binomial distribution for the Poisson process, and is a handy thing to remember. This is the origin of my comment of last October, that if there is an expected number p, then the probability of getting no counts is e^{-p} , so that the probability of having a detection (one or more counts) is $(1-e^{-p})$.

So if we call the "high" efficiency of counters A and B (each) ϵ_1 , then the probability of a count (for a single injected neutron) in A or in B is just ϵ_1 . The expected number of counts in either of these counters for M neutrons injected is $M\epsilon_1$ (each), so the probability of no detection in counter A is $e^{-(M\epsilon_1)}$. Therefore, the probability of a vote from counter A is $(1-e^{-(M\epsilon_1)})$. And the probability of a vote from A and from B is the square of this value.

In similar fashion (and particularly suitable for calculation without paper), one can write the probability that the gang of ten will give two votes or more as one minus the probability that the gang of ten will give zero votes, minus the probability that it will give precisely a single vote. The probability that a single counter will give zero votes is $e^{-(M\epsilon_2)}$, which we can call "NS" for "no". The probability that it will give vote is then one minus this value $(1-NS)$, which we can call "YS" for "yes". Therefore, the probability that there will be no votes among the N (which is 10) is NS^N , and the probability that there will be precisely one vote among the N is $N(YS/NS)*(NS^{N-1})$.

I believe the "number of votes" is precisely K, in your notation. In order to avoid confusion, I will let P (K,M) represent the probability of obtaining K or more votes from a burst of M neutrons. From what I have said before

$$P(K,M) = P_2(K,M) + P_1(K,M) + P_1(K,M) + P_0(K,M)$$

where the first term means detection in both A and B, the second and third terms detection in A or in B respectively, and the fourth term detection in neither A nor B.

$$R \quad P_2(K,M) = ((1-e^{-(M\epsilon_1)})^2) * (1-(1+N*YS/NS)*(NS^{N-1}))$$

where $NS = e^{-M \cdot \epsilon_2}$ and $YS = 1 - NS$.

For $K = 4$ and $M = 22$, we have $P2(K,M) = 0.3929$.
 Similarly, $P1 = 0.02071$ and $P0 = 0.00077$, for a grand total
 $P2 + P1 + P1 + P0 = 0.4351$.

Neutrons into assemblage:

2	4	6	8	10
12	14	16	18	20
22	24	26	28	30
32	34	36	38	40
42	44	46	48	50

Probability of 4 or more votes (P):

0.0004	0.0055	0.0206	0.0483	0.0880
0.1375	0.1939	0.2541	0.3157	0.3764
0.4351	0.4905	0.5422	0.5899	0.6335
0.6731	0.7089	0.7412	0.7701	0.7960
0.8191	0.8397	0.8581	0.8744	0.8889

I have calculated precisely the results of this algorithm by a program in the APL language. However convenient and compact, this is neither readily interpreted nor reproduced in ordinary fonts. I will send you and Steve the APL program for reference (by FAX). In the meantime, I attach a page of results.

Note that even a single neutron has a (very small) probability of giving 4 votes. Aside from this partially non-physical aspect, I believe the results obey reasonable limits. If one insisted that a neutron could give only a single vote, the calculated efficiencies could be taken to apply to neutron numbers M larger than those tabulated by a factor 1.18 for $P2$ and $P0$; 1.08 for $P1$ and $P0$, $P2RP$, and $P4RP$. I hope these calculations are helpful and would welcome any detection of errors. Please note that the table for P differs from that I sent yesterday-- I found an error in one of the formulas for $P1$.

Sincerely yours,

Richard L. Garwin

cc:

S.E. Jones, BYU (Via BITNET to JONESSE at BYUWAX).

RLG:jtml:P024MG:012490..MG

'PROBABILITIES ARE NAMED AS FOLLOWS:'
 'P2 IS BOTH A AND B, AND 2 OR MORE OF 10 RING COUNTERS'
 'P1 IS A AND NOT B, AND 3 OR MORE OF 10 RING COUNTERS'
 'P0 IS NEITHER A NOR B, AND 4 OR MORE OF 10 RING COUNTERS'
 'P IS P2 + P1 + P1 + P0, SO 4 OR MORE COUNTERS OF ANY TYPE'
 'P2RP IS 2 OR MORE OF 10 RINGERS, INDEPENDENT OF A AND B'
 'P4RP IS 4 OR MORE OF 10 RINGERS, INDEPENDENT OF A AND B'

(Neutrons injected)					*** Numeric M ***
2	4	6	8	10	
12	14	16	18	20	
22	24	26	28	30	
32	34	36	38	40	
42	44	46	48	50	

					*** Numeric P2 ***
0.0003	0.0041	0.0156	0.0376	0.0705	
0.1130	0.1629	0.218	0.276	0.3348	
0.3929	0.449	0.5024	0.5524	0.5988	
0.6415	0.6804	0.7157	0.7476	0.7763	
0.8019	0.8249	0.8453	0.8635	0.8796	

					*** Numeric P1 ***
0.00006	0.00070	0.00238	0.00508	0.00841	
0.01187	0.01501	0.01756	0.01936	0.02039	
0.02071	0.02041	0.01964	0.01852	0.01717	
0.01568	0.01415	0.01263	0.01117	0.00979	
0.00852	0.00737	0.00634	0.00543	0.00463	

					*** Numeric P0 ***
0.00000	0.00007	0.00024	0.00046	0.00068	
0.00085	0.00095	0.00097	0.00094	0.00087	
0.00077	0.00066	0.00055	0.00045	0.00036	
0.00028	0.00021	0.00016	0.00012	0.00009	
0.00007	0.00005	0.00003	0.00002	0.00001	

					*** Numeric P ***
0.0004	0.0055	0.0206	0.0483	0.0880	
0.1375	0.1939	0.2541	0.3157	0.3764	
0.4351	0.4905	0.5422	0.5899	0.6335	
0.6731	0.7089	0.7412	0.7701	0.796	
0.8191	0.8397	0.8581	0.8744	0.8889	

					*** Numeric P2RP ***
0.0104	0.0377	0.0769	0.1242	0.1764	
0.2313	0.287	0.3423	0.3961	0.4478	
0.4969	0.5431	0.5862	0.6263	0.6632	
0.6972	0.7282	0.7565	0.7822	0.8055	
0.8266	0.8455	0.8626	0.8779	0.8916	

					*** Numeric P4RP ***
0.0000	0.0001	0.0008	0.0022	0.0050	
0.0093	0.0156	0.0240	0.0347	0.0477	
0.0630	0.0806	0.1004	0.1221	0.1456	
0.1707	0.1971	0.2247	0.2531	0.2823	
0.3118	0.3417	0.3715	0.4013	0.4308	

(11)

.fo off fold

$$P(K,M) = P_2(K,M) + P_1(K,M) + P_1(K,M) + P_0(K,M)$$

.sk

where the first term means detection in .us on;both ;.us off; A and B,
the second and third terms detection in A or in B respectively, and the
fourth term detection in .us on;neither ;.us off; A nor B.

.sk

$$P_2(K,M) = ((1 - \exp^{-M \cdot \epsilon_2})^2) * (1 - (1 + N \cdot y/n)^{-n \cdot N})$$

.sk

where $n = \exp^{-M \cdot \epsilon_1}$ and $y = 1 - n$.

.sk

For $K = 4$ and $M = 22$, we have $P_2(K,M) = 0.3929$.

Similarly, $P_1 = 0.0395$ and $P_0 = 0.0045$, for a grand total

$$P_2 + P_1 + P_1 + P_0 = 0.4764.$$

.sk

Neutrons into assemblage:

2	4	6	8	10
12	14	16	18	20
22	24	26	28	30
32	34	36	38	40
42	44	46	48	50

Probability of 4 or more votes (P):

0.0071	0.0271	0.0584	0.0992	0.1473
0.2004	0.2563	0.3132	0.3695	0.4242
0.4764	0.5257	0.5716	0.6142	0.6534
0.6892	0.7218	0.7514	0.7782	0.8023
0.8241	0.8436	0.8611	0.8767	0.8907

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for
input

New Energy Times Archive

90/01/23 17:03:45 *** Function GAI ***

```

∇ GAI
[1] PA←1-*(-M×EPS1)
[2] NS←*(-M×EPS2)
[3] YS←1-NS
[4] P2←(PA*2)×(1-(NS*N)×(1+N×YS÷NS))
[5] P2
[6] P1←PA×(1-PA)×1-(NS*N)×TMP←1+(N×YS÷NS)+N×(N-1)×(1÷3×2)×(YS÷NS)*2
[7] P0←((1-PA)*2)×1-(NS*N)×TMP+N×(N-1)×(N-2)×(1÷4×3×2)×(YS÷NS)*3
[8] P←P2+P1+P1+P0
[9] P
∇

```

$$N = 22$$

$$K = 4$$

$$P_2 = 0.3549$$

$$P_1 = 0.0395$$

$$P_0 = 0.0045$$

$$P = P_2 + P_1 + P_1 + P_0 = 0.4764$$

(1.7)

Date: 23 January 1990, 17:54:18 EST

From: (R.L.Garwin (914) 945-2555) RLG2 at YKTVMV
IBM Fellow and Science Advisor to the Director of Research
P.O. Box 218
Yorktown Hts, NY 10598

To: GAI at YALEVM

cc: JONESSE at BYUVAX

Subject: Counting efficiency.

Reply-To: RLG2 at WATSON

I have calculated as promised. I will send the results tomorrow morning, together with a detailed exposition.

You will be able to see exactly the assumptions made and will be able to calculate for other values.

Dick Garwin

New Energy Times Archive

012390.M62

1.8

Date: 23 January 1990, 18:39:09 EST

From: (R.L.Garwin (914) 945-2555) RLG2 at YKTMV
 IBM Fellow and Science Advisor to the Director of Research
 P.O. Box 218
 Yorktown Hts, NY 10598

To: GAI at YALEVM

cc: JONESSE at BYUVAX

Subject: FAX, etc.

Reply-To: RLG2 at WATSON

I just received your FAX, which I will take home to study.

Here is the (probably not understandable) meat of the letter. Letter is dictated and has more equations, but this I did myself in order to incorporate the results of calculations.

* *****

* $P(K,M) = P_2(K,M) + P_1(K,M) + P_1(K,M) + P_0(K,M)$

* .sk

* where the first term means detection in .us on; both ; .us off; A and B,
 * the second and third terms detection in A or in B respectively, and the
 * fourth term detection in .us on; neither ; .us off; A nor B.

* .sk

* $P_2(K,M) = ((1 - \exp^{-M \cdot \epsilon_2})^2) * (1 - (1 + N \cdot y/n)^{-n \cdot N})$

* .sk

* where $n = \exp^{-M \cdot \epsilon_1}$ and $y = 1 - n$.

* .sk

* For $K = 4$ and $M = 22$, we have $P_2(K,M) = 0.3929$.* Similarly, $P_1 = 0.0395$ and $P_0 = 0.0045$, for a grand total* $P_2 + P_1 + P_1 + P_0 = 0.4764$.

* .sk

* Neutrons into assemblage:

2	4	6	8	10
12	14	16	18	20
22	24	26	28	30
32	34	36	38	40
42	44	46	48	50

* .sk

* .sk

* Probability of 4 or more votes (P):

* .sk

* 0.0071 0.0271 0.0584 0.0992 0.1473

* 0.2004 0.2563 0.3132 0.3695 0.4242

* 0.4764 0.5257 0.5716 0.6142 0.6534

* 0.6892 0.7218 0.7514 0.7782 0.8023

* 0.8241 0.8436 0.8611 0.8767 0.8907

* .sk

* .fo on

ring cubes and have expected hits $\overline{m} = N \cdot \epsilon_2$.

$$NS = e^{-\overline{m}} \quad (YS) = (1 - NS)$$

probability of N times giving K heads
 Morgan & Mayhew paper (1953)
 $w_n(m) = \binom{n}{m} p^m q^{n-m}$

012390.MG3

TITLE: UPDATE ON THE MEASUREMENT OF NEUTRON EMISSION
FROM Ti SAMPLES IN PRESSURIZED D₂ GAS

AUTHOR(S): H. O. Menlove, Eduardo Garcia, and S. E. Jones

SUBMITTED TO: NSF-EPRI Workshop on "Anomalous Effects in Deuterated Metals"
Washington, DC
October 16-18, 1989
(FULL PAPER)

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Los Alamos Los Alamos National Laboratory
Los Alamos, New Mexico 87545

UPDATE ON THE MEASUREMENT OF NEUTRON EMISSION FROM Ti SAMPLES IN PRESSURIZED D₂ GAS

H. O. Menlove and E. Garcia
Los Alamos National Laboratory
Los Alamos, NM 87545

S. E. Jones
Brigham Young University
Provo, UT 84602

During the Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, May 2, 1989, we reported [Los Alamos National Laboratory report LA-11686-C (September 1989)] on the measurement of neutrons emitted during pressurized D₂ gas experiments using Ti and Pd samples. The experimental program has continued since the Santa Fe meeting, and our data base has more than doubled.

Our recent work has included detector upgrades, background investigations, acoustical emissions, and sample preparation and procedure investigations. This report will give a brief summary of our work in the above areas.

I. DETECTOR UPGRADES

We are using four independent ³He neutron detector systems for our experiments. All of the systems use ³He tubes in polyethylene (CH₂) moderators to detect the neutrons. The detector characteristics and background levels are given in Table I.

TABLE I. Neutron Detector Characteristics

Identification	Shape	Number ³ He Tubes ^a	Total Efficiency ^b	Random Bkg (counts/s)	Coincidence Bkg ^c (counts/h)
System 1	Rectangular channel	18	21	0.23	1.3
System 2	Cylindrical cavity	6	26	0.092	0.35
System 3	Cylindrical cavity	16 + 5	34	0.16	2.3
System 4	Cylindrical cavity	16 + 3	31	0.39	2.3

^aThe ³He tubes typically have a 30 cm active length and a fill pressure of 4-6 atm.

^bThe total efficiency was measured using a calibrated ²⁵²Cf source located at the sample position.

^cThe coincidence background was measured with a 1.2-kg sample cylinder in the counting position.

A. Background Neutrons

We have reported results for both random neutron (totals) emission and time-correlated (coincidence) neutron emission. The background random neutrons originate primarily from cosmic-ray interactions in the detector, shielding, and sample, as well as from radioactive decay of uranium in the concrete shielding and α and β decay in the ^3He tube walls. On the other hand, the time-correlated neutrons originate from cosmic-ray spallation reactions in the sample and detector body. If the spallation reaction takes place at a significant distance from the detector tubes, the small solid angle and intervening shielding between the spallation and the detector prevent the detection of a coincidence event.

To investigate neutron background fluctuations, we counted the neutron backgrounds from the dummy sample for 3 weeks for System 4 and 2 weeks for System 3 and showed that the observed scatter of random counts (10^4 -s time intervals) about the mean was no larger than the expected standard deviations (RSD) of 1.6% for System 4 and 2.0% for System 3.

Recently, the external neutron backgrounds in Systems 1, 2, and 3 have been reduced by the addition of 10 to 30 cm of CH_2 to the exterior of the detectors. These lower rates are reflected in Table I.

B. System 3 Upgrade

An electronic noise veto counter has been added to System 3. Figure 1(a) shows a schematic diagram of the upgraded system. The split of the high voltage to the external set of five ^3He tubes is used to pick up possible electronic noise such as voltage spikes and rf interference. The external tubes have no CH_2 and they are wrapped in cadmium so they have negligible efficiency for counting neutrons. Their coincidence rate is less than 1 count per day. A coincidence count in this external counter vetoes a count in any of the other systems.

C. System 4 Upgrade

A third ring of ^3He tubes has been added to System 4 as shown in Fig. 1(b). Ring 3 has completely independent electronics from the two inside rings and it measures the sample neutrons with an overall efficiency of 5%. The ratio of totals counts between the inside detector and ring 3 is 6/1 and the corresponding coincidence count ratio is 46/1.

The ring ratio can be used to show that the neutrons originated from the sample, and for larger bursts, the ratio can be used to establish the average neutron energy.

D. Neutron Die-away Time

For System 4, we have split the output from the inside detector to two different coincidence electronics, as shown in Fig. 1(b). The normal shift-register¹ has a coincidence gate of 128 μs and the second shift register has a gate of 32 μs . A ^{252}Cf source was used to show that an instantaneous burst of neutrons results in a coincidence ratio in the two gates (128 μs / 32 μs) of 2/1. We now check that our background and sample burst events satisfy this ratio within the counting statistics. This is essentially a measure of the neutron die-away time ($\sim 50 \mu\text{s}$) in the detector.

Typical sources of electronic noise will not satisfy this gate ratio criteria.

II. NEUTRON BURST RESULTS

We have continued to measure neutron bursts from the Ti samples in D_2 gas; however, the events are unpredictable and difficult to obtain. Table II gives a list of the samples that have yielded neutron bursts. The list shows the number of source neutrons, and the underlined values correspond to events that occurred during warm-up from liquid nitrogen (LN) temperatures at

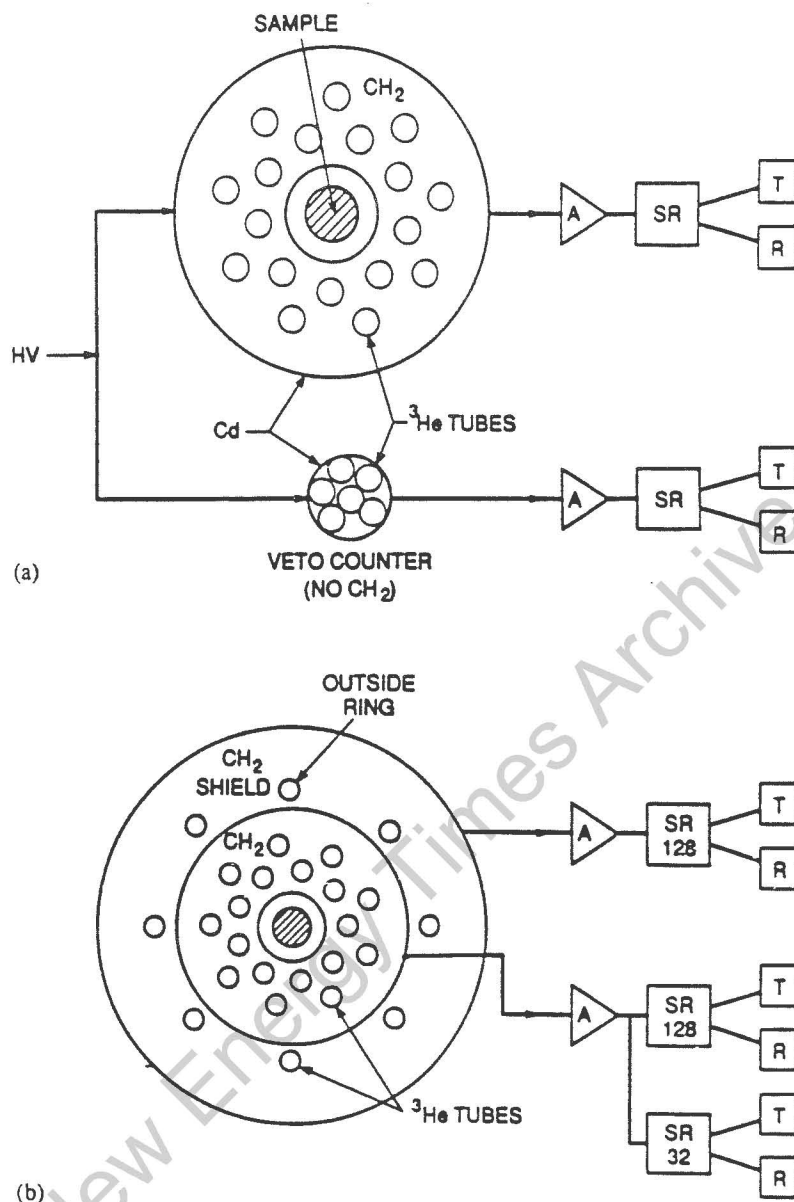


Fig. 1. (a) Schematic diagram of detector System 3 with the external electrical noise veto counter containing five ^3He tubes inside a cadmium absorber. (b) Schematic diagram of detector System 4 with an outside ring of detectors for splitting the signal from the inside detector and dual gate electronics for the inside tubes.

approximately -30°C . The samples in parenthesis were marginal performers, and they are on the borderline between active and inactive samples.

Table III lists the samples that were inactive in that they yielded no neutron bursts. Four of the inactive samples were H_2 gas control samples and six were deuterided Pd samples. All of the H_2 samples have been inactive and they are currently being used for the dummy samples.

TABLE II. Active Samples--Burst Results

Starting Date (Mo/d)	Sample ^a	Number of Source Neutrons ^b
4/28	Ti-1	<u>127</u> , <u>136</u> , <u>85</u> , <u>76</u>
5/6	Ti-6	<u>258</u> , <u>179</u> , <u>121</u> , <u>70</u> , <u>15</u> , <u>127</u> , <u>94</u>
5/16	(Ti-10)	<u>12</u> , <u>15</u>
5/19	DH-1	55, 15, 15, 27
6/2	DD-2	12, 85, <u>15</u> , <u>15</u> , <u>39</u> , <u>142</u> , 18, 30
6/9	(DH-4)	21
6/12	(Ti-13)	24
6/20	Ti-14	15, 91, <u>12</u> , 24
6/28	Ti-16	24, 24, 12, <u>15</u> , 18, 15, 15, <u>109</u> , <u>18</u>
7/8	DD-56	<u>12</u> , <u>279</u> , <u>130</u> , <u>88</u> , <u>48</u>
7/18	Ti-19	88, 15
8/18	Ti-22	55, <u>97</u>
8/11	(Ti-23)	19, 23, 19
8/8	Ti-24	35, <u>119</u> , <u>16</u> , 16, 16, 15, 15
8/22	(Ti-25)	32
8/24	(Ti-28)	<u>17</u>
8/29	Ti-30	<u>280</u> , <u>13</u>
9/11	Ti-31A	<u>86</u> , <u>34</u>
9/12	Ti-32	<u>23</u> , 35

^aThe parenthesis around a sample number indicates a marginal performer that is on the borderline between active and inactive samples.

^bThe underline under the neutron yield indicates that the burst occurred during warm-up from LN temperature.

Most of the samples from Ti-12 to Ti-36 have used variations of the alloy Ti-6, 6, 2 (6% Al, 6% V, 2% Sn) or Ti-6, 4 (6% Al, 4% V). The metal bar stock is cut into small turnings (~1 by 1 by 3 mm) on a lathe.

An example of an active sample data run is shown in Fig. 2, where the coincidence counts per 2000-s time interval are plotted vs the counting time. The large emission at about 195 h occurred at the -30° temperature after removing the sample from the LN following a D₂ gas refill.

Table III. Inactive Samples

H ₂ Gas Controls	D ₂ Gas Ti Samples	D ₂ Gas Pd Samples
Ti-27	Ti-2	Pd-1S
Ti-29	Ti-4	Pd-2S
Ti-31	Ti-5	Pd-3S
Ti-35	Ti-7	Pd-4S
	DD-3	Pd-5L
	Ti-8	
	Ti-9	
	Ti-11	
	Ti-12	
	Ti-15	
	Ti-17	
	Ti-18	
	Ti-20	
	Ti-21	
	Ti-26	

The relationship between the neutron bursts and the LN temperature cycle² continues. The underlined values in Table II show that the majority of neutron bursts occur during warm-up from LN; however, this time period while the sample is between -100°C and 0°C represents less than 5% of the sample counting time. The dummy samples and control samples have *never* given a burst during their equivalent warm-up period.

This dramatic relationship between the LN cycle and the bursts is illustrated in Fig. 3, where the burst frequency is plotted as a function of the number of source neutrons. We see that there are a few events as large as 200-300 source neutrons; however, most events are at the low end of the distribution. Sources smaller than ~15 n are lost in the cosmic-ray spallation background.

In Fig. 3, the solid bars represent bursts that occurred at ~-30°C, and the cross-hatched bars correspond to room temperature bursts. It is highly significant that *all* of the high-yield bursts occurred during the LN warm-up at ~-30°C.

For recent experiments, we have attached an acoustic emission sensor³ to the outside of our stainless-steel cylinders that contain the D₂ gas and Ti chips. The sensor can detect the cracking sounds from the hydriding of the Ti metal chips.

We have found that there is no detectable acoustical emission at the time of the neutron bursts. However, samples that have no- or low-acoustical emission are not likely to yield neutrons. Samples that we thought were the same yielded acoustical emissions that differed by an order of magnitude. Several samples have yielded many thousands of acoustical emissions without any neutron emission.

III. SUMMARY

We continue to observe infrequent neutron bursts from Ti metal chips and electrolysis residue samples in pressurized D₂ gas. We have been unable to pinpoint the sample characteristics that yield the neutrons. However, we have identified some negative characteristics related to inactive samples such as (1) predeuterided Ti or Pd at high temperature, (2) unstressed Ti pieces that do not crack under the LN temperature cycle. The H₂ control samples have all been inactive.

Ten samples have yielded large neutron bursts with at least 23 bursts at the ~-30°C temperature. A time interval of several days is required between the gas loading and the first neutron yield from the LN cycles. High-efficiency detectors, time-correlation counting, and noise rejection are required to measure the neutron bursts.

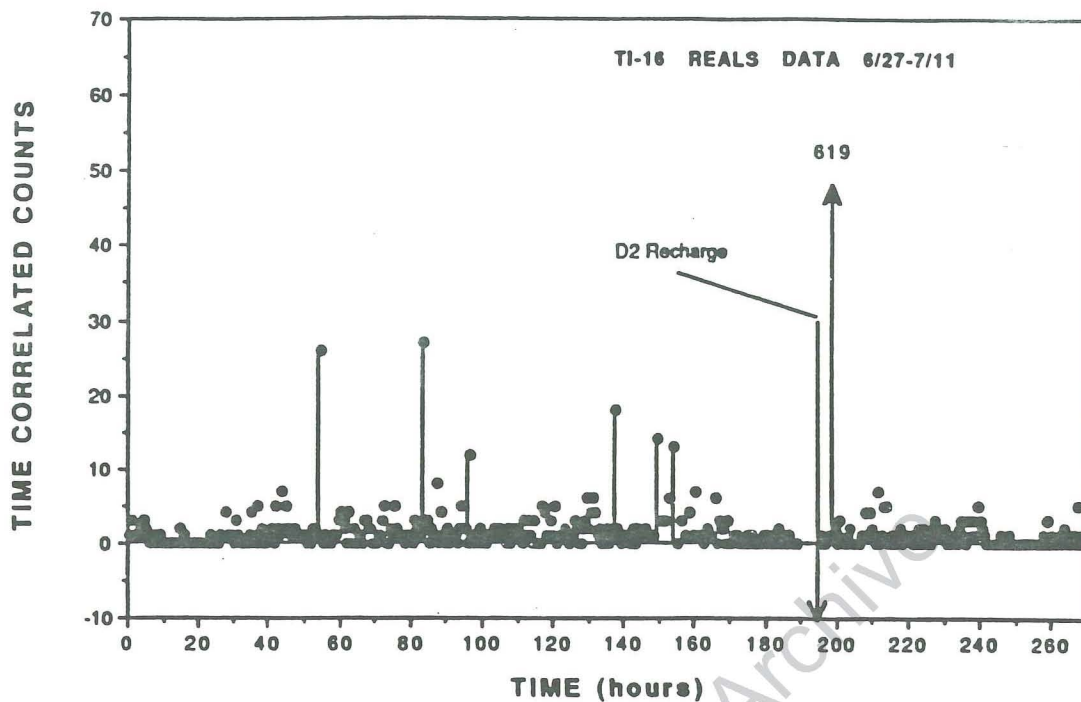


Fig. 2. Neutron time-correlated counts for 2000-s counting intervals vs time for Ti-16. The large burst at 195 h into the experiment occurred during warm-up from LN at $\sim -30^{\circ}\text{C}$ following a refill of D_2 gas in the cylinder.

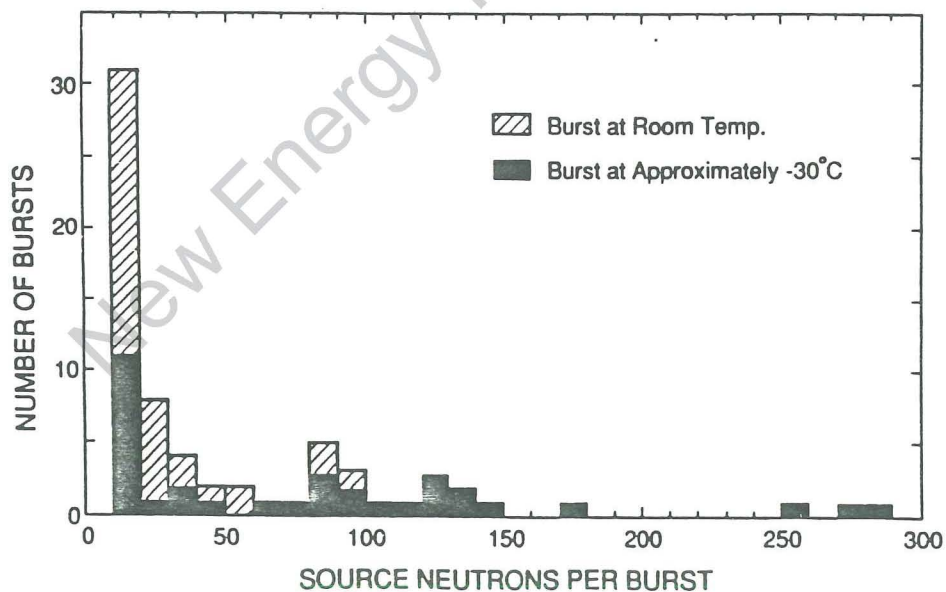
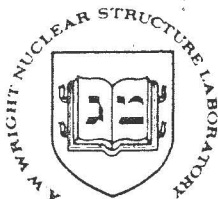


Fig. 3. The neutron burst frequency vs the yield of source neutrons in the burst. The solid bars correspond to burst events during the LN warm-up at $\sim -30^{\circ}\text{C}$.

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2. H. O. Menlove, M. M. Fowler, E. Garcia, A. Mayer, M. C. Miller, R. R. Ryan, and S. Jones, "The Measurement of Neutron Emissions from Ti plus D₂ Gas," in *Highlights of Papers Presented at the Workshop on Cold Fusion Phenomena*, Santa Fe, New Mexico, May 23-25, 1989, Los Alamos National Laboratory report LA-11686-C (September 1989), p. 13.
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R.L. GARWIN-

1.4

MEMORANDUM

January 17, 1990

TO: Steve Jones

FROM: Moshe Gai

SUBJECT: 1. Derivation of Moshe's efficiency formula (from report to DOE)
2. Comment by Dick Garwin

I have mailed to you the three papers where the most general case for hit probabilities are derived. If you work it out you will convince yourself that Moshe's formula from the interim report is a 1st order approximation of the most general solution, as I shall demonstrate here. The papers I listed are:

1. G.B. Hagemann et al.; Nucl. Phys. A245(1975)166.
2. O. Andersen et al.; Nucl. Phys. A295(1978)163.
3. L. Westerberg et al.; Nucl. Instr. Meth. 145(1977)295.

At first you will note that in Ref. 1 equations A.2 should be multiplied by the binomial coefficient $B(N,p)$ on the LHS and equ. A.3 should be multiplied by the same binomial on the RHS. This is correctly done in equ. 15 of Ref. 3. Please note that k of Ref. 3 is equal to $N-x$, of Ref. 1. This new notation was introduced in Ref. 3 so as not to sum binomial coefficients of value zero.

The most general solution was given by B.R. Mottelson (see p. 186 of Ref. 1), in equ. 14 of Ref. 1, which is the same equation as A.3. The hit probability is then:

$$P_{Np} = (-)^{N+p} \sum_{x=1}^N (-)^{x+1} * B(p, N-x) F_x * B(N, p)$$

In Ref. 1 we find: "To the first order in Ω^p , eq. (14) gives:"

$$= M * (M-1) * \dots * (M-p+1) \Omega^p * B(N, p)$$

You will note that since we don't run our array in coincidence with a gamma detector: $(M-1)$ of Ref. 1 $\rightarrow M$, as it appears here. But:

$$= M * N \Omega * (M-1) * (N-1) \Omega / 2 * \dots * (M-p+1) * (N-p+1) \Omega / p$$

Which is the so called Moshe's formula (actually was discovered by many nuclear physicist before me). I shall state again, there is nothing wrong with Moshe's formula as appeared in the report. This formula is clearly an approximation to the exact solution, which was appropriate for an interim report. As such it is

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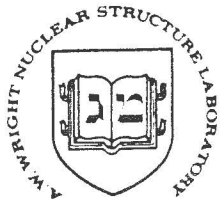
bound to break down (when the conditions for the approximation are not met). You will note that both "Moshe's formula" as appeared in our report, and equ. 15 of Ref. 1 yield efficiencies which are larger than one, for some value of M . i.e. at these values of M the approximation breaks down. A quick glance in Mottelson formula and Ref. 1 would have saved a lot of time.

2. Dick Garwin comment to me was of completely different nature, and I am afraid Steve you did not understand it. He correctly states, that if you have say a probability p of winning, if you do an experiment $1/p$ times that still does not mean that you will arrive at 100% winning probability, since the distribution is of Poisson type. This comment has nothing to do with the approximation introduced in Moshe's formula.

I hope that this finally settles these trivia!

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CONFIDENTIAL



YALE UNIVERSITY

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P.O. Box 6666, 272 Whitney Avenue, New Haven, Connecticut 06511

Dr. John Maddox
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London, WC2R 3LF
United Kingdom

January 10, 1990

Manuscript #: M06594R
H.O. Menlove et al.

Dear Dr. Maddox,

On October 19th, 1989, I offered my resignation as a referee on the above paper submitted by H.O. Menlove et al., a Los Alamos - Brigham Young University collaboration on "cold fusion". I cited a "conflict of interest" as the reason for my resignation, as I was at that time involved in a collaboration with Professor Steve E. Jones of BYU, to test the validity of the experimental results reported in the paper by Menlove et al. In retrospective my resignation, which at the time appeared to be the right thing to do, was a mistake. I now feel that in fact due to my collaboration with Steve E. Jones, I am in a better position for judging the quality of that paper and would like to offer you some additional information on this work.

I feel that throughout my dealing with the Los Alamos-BYU collaboration, as a referee, and in endless conversations and communications with Dr. Jones and Dr. Menlove, the collaboration has failed to address the specific criticisms raised about their paper and their experiment. It seems that they have neglected to reply, as I shall detail below, assuming that the criticism will just vanish. I am writing to you directly, because I fear the process involved runs deeply against one of the pillars of the scientific method: "the scientific debate", a process at which scientific criticism is directly addressed and properly dealt with.

As a referee I criticized the paper of Menlove et al. and pointed out shortcomings that could be addressed in simple new experiments. Instead of addressing these shortcomings with new data, Menlove et al. have apparently continued to search for "cold fusion" neutrons over the last half a year or so, in the same old way that they have been doing it, without altering their scientific method and without addressing my criticism.

Specifically, I pointed out the fact that the Menlove experiment has a large background, some factor of 20 to 2000 larger than any of the other "cold fusion" searches that were active at the time. I understood from Howard Menlove, that the experiment could be repeated in a quieter environment, in a nearby cave in Los Alamos laboratory, but this to my knowledge was not done. With such a large background I really don't know what they are measuring, and the bursts could arise from a lot of

possibilities; noise, MeVs electrons from cosmic shower that will activate the ^3He counter, neutrons from cosmic showers, etc. etc.

I noted that the control experiments were insufficient to show that a real effect was observed. In many searches for "cold fusion" neutrons, other researchers observed such bursts with the control sample, and much care is required to eliminate false detections. As you know in my study of the background of the Yale-BYU collaboration, a burst of neutrons from a control cell was observed, including some 20 neutrons. In our experiment the background is some factor of a 1000 times smaller than the Los Alamos-BYU one, but we still observe such burst events in the background. In this Los Alamos experiment, due to the large background, several additional control experiments must be carried out, for example with deuterium gas no Ti, or with various shielding to study the effect of a cosmic shower etc. etc., as detailed in my two reports to Nature from last summer. To my knowledge none of these control experiments were carried out. In my judgement the paper by Menlove does not provide convincing evidence for observing a real burst of neutrons and excluding background effects.

In the search for the random emission of neutrons, while the data were measured over one night, the background was measured over a different night, and simple fluctuation in barometric pressure between the two nights, could account for the 10% excess of neutrons above "background" reported by Menlove et al. The experiment performed here is incomplete in ruling out this possibility, even though a very simple experimental setup could be devised to address this issue correctly. As detailed in my report(s) the method used in the paper is in my opinion wrong on this aspect. I am surprised that after we had such a well publicized dialogue in Nature, back in May, lead by the comments of John Carpenter, on the effect of barometric pressure changes on cosmic neutron flux, the Los Alamos-BYU experiment still neglected this effect.

I have pointed out their over optimistic estimate of error bars, where they neglected systematical errors. The authors quote an error bar of the order of 0.5%, where I showed that merely the placement of the cylinder in the detector would cause a larger error bar. The authors to my understanding never in fact addressed this issue and continued to quote a statistical significance of their effect of some 10σ or so, which I have shown to be manifestly wrong.

In their manuscript they claim that bursts of neutrons are observed at a specific temperature of the cylinder, at -30°C , as the cylinder is brought to room temperature from liquid nitrogen temperature. This is in fact not supported by their data. Unfortunately, the collaboration does not have a detailed table including all the parameters of the bursts of neutrons that they observed. When I mentioned to Dr. Jones that in fact in the Menlove paper submitted to Nature, there are quite a few bursts shown to appear at room temperature, Dr. Jones in fact revealed that 40% of the bursts were obtained at room temperature. Dr. Jones does not see this as contradicting his statement, that the bursts are distributed mainly around -30°C , since the time they spent measuring at room

temperature is in fact much longer, so in his opinion they simply sampled the tail of the distribution over a longer time. Neglecting the logical structure of this argument, I have suggested to Dr. Jones a simple test of this idea by measuring only at -30°C , on top of the alleged distribution, and therefore increase the neutron yield. Dr. Jones replied that they did not do it since they claim that the cylinders have to soak in deuterium gas at room temperature for a long time, before neutron emission starts... This sounds to me like an ad-hoc theory on deuteriding, and I was unable to confirm it with any of my material science colleagues. Furthermore, this new "soaking procedure" is also contradicted by the data shown in Menlove et al, i.e. in Table III 50% of the bursts described appear before cycle 5 (within a few days) and all bursts appear before cycle 9 (slightly over a week). I should mention that many of these communications with Dr. Jones, were in writing and I would be happy to provide Nature with any of Dr. Jones' memoranda.

While collaborating with Dr. Jones the material science experts from Yale, reviewed the protocol developed in Los Alamos for treating the Ti samples. To our surprise we learned that the Ti samples were incorrectly handled in the Los Alamos experiment and without the removal of the surface oxide layer it is in fact possible that no deuterium have penetrated the Ti samples used in Los Alamos! A similar statement was made by the experts in Brookhaven, who studied deuteriding of Ti in the last 20 years as part of the 'hydrogen economy program'. In my talk at the "New York Material Science Society", on Nov. 2nd, this neglect in the Los Alamos experiment was met with a worry about the relevance of these results to the so called "cold fusion" phenomena. This fact escaped the criticism of my original referee report, due to insufficient details provided in the Menlove paper about the treatment of the Ti samples.

Unfortunately, the Los Alamos-BYU collaboration did not address this new criticism on the material science aspects of their experiment, by repeating the experiment with a treated surface, that should allow the formation of the deuteride phase. Even though one may imagine that this treatment of the surface would yield an increase in neutron emission. While one may claim that they still found an altogether new phenomena not related to "cold fusion", these problems should be clearly stated in the paper.

Instead of addressing the criticism on the treatment of the Ti, the authors have stated an ad-hoc theory that the deuterium is inserted to the metal through repeated cycling to liquid nitrogen temperatures, by cracking the surface. They claim that after the surface is cracked, the deuterium makes its way to the bulk of the metal over a prolonged period of time through the fresh cracks in the surface. Again, I was unable to verify this theory for "a new method" for deuteriding Ti, with any expert on material science. Furthermore, they all tell me that once the deuteriding process starts the metal is brought to saturation almost immediately, contrary to the claims of Jones et al.

All these arguments on the metal characterization could be addressed by simple measurements (i.e. pressure measurements) and by using standard procedures for material characterization. I am frustrated that after half a year or so of work, the laboratory at Los Alamos has neglected to characterize the Ti samples used in their work. We still have no quantitative evidence whatsoever, that the Ti samples in the Los Alamos work included deuterium in any part of the metal! I feel that this is an important issue that the authors should address.

As you know the Yale-BYU-BNL negative results, appearing in a joint report submitted on Oct. 27th to the DOE/ERAB panel on "cold fusion", are now almost five months old. I maintain that our data invalidate the Los Alamos-BYU results as appearing in the manuscript submitted to Nature. While I don't claim that one search should prohibit the publication of another, and again steering away from "conflict of interest", the result of our collaboration only emphasize the validity of the criticism against the Los Alamos-BYU paper.

It is important to mention the results of the Yale-BYU-BNL collaboration since I noticed an effort to delay the publication of our data and to down play our results. On Nov. 9th, 1989, Nature have published quotations from Dr. Jones on our collaboration which contradict scientific facts appearing in our report to DOE. Later Dr. Jones in a communication to Dr. Lindley and to myself, suggested that he was misquoted by Nature. Last Friday when I requested that Dr. Jones participates in a three way conference call including Dr. Lindley, Dr. Jones and myself, to resolve the matter, Dr. Jones retracted his previous statement that he was misquoted. But the fact still remains unchanged namely, a false criticism of the Yale-BYU-BNL experiment was published in Nature.

In the Nature comment Jones claims that the Yale-BYU-BNL experiment that lasted 10 days, was too short to allow the formation of the deuteriding phase. Again as mentioned before, the data in Table III of Menlove et al., contradict this statement (with 50% of the bursts occurring within a few days). While we have exactly repeated the Los Alamos procedures, including the length of the experiment and the number of cycles, as reported in the paper by Menlove et al. submitted to Nature, we find negative results with upper limits on neutron emission over an order of magnitude smaller than reported in Los Alamos, as quoted in our report to DOE. The sensitivity of the Yale experiment is much improved due to a smaller background with a similar detection efficiency.

It is my opinion, that the severe criticism of the Los Alamos-BYU experiment as outlined above (coupled with the negative results of the Yale-BYU-BNL experiment) invalidate the paper of Menlove et al. and the burden is now on the Los Alamos-BYU collaboration to prove that they performed a measurement of some relevance. This I feel, is certainly not

demonstrated in the manuscript submitted to Nature, and can only be addressed by performing the appropriate future experiments that will demonstrate their claims to be true.

Sincerely,

Moshe Gai

Moshe Gai
Associate Professor of Physics

cc: Dave Lindley
Phillip Ball.

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GAMMA DECAY AFTER HEAVY ION REACTIONS STUDIED BY FIRST AND HIGHER ORDER MULTIPLICITY MEASUREMENTS

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Received 23 December 1974

(Revised 7 March 1975)

Abstract: First and higher order multiple coincidence rates have been measured for the γ -rays from ^{16}O induced reactions on targets of $^{148,150}\text{Nd}$ at various bombarding energies. Average values and shape parameters for the γ -ray multiplicity distributions have been extracted from the data. The multiplicities are connected to angular momentum through complete fusion cross sections, and conclusions are drawn on spin distributions of entry states in the final nuclei as well as on the decay pattern from these states.

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NUCLEAR REACTIONS $^{148,150}\text{Nd}(^{16}\text{O}, xny)$, $E_{^{16}\text{O}} = 65\text{--}84\text{ MeV}$; measured γ -ray multiplicities. Enriched targets.

1. Introduction

(Heavy ion, xny) reactions have in recent years been used extensively for the study of nuclear properties of high spin rotational and vibrational states. In fact, the knowledge existing about the low-lying rotational bands, up to the highest known spin values, is obtained mostly through such reactions.

From simple classical estimates as well as from more advanced optical model calculations of reaction cross sections, one can predict the range of angular momentum input which can be reached in a specific experiment. Although angular momentum input as high as $40\hbar$ can be obtained in many laboratories, only very few states with spin higher than 18–20 have been established, and $I = 22$ is so far the maximum value.

This apparent gap between the established spin values and the expected angular momentum input is not fully understood at present. The subject is discussed by Newton *et al.*¹⁾ in connection with the feeding times of the ground state band. Sunyar and his co-workers²⁾ have discussed additional information obtained about this gap, referred to as the continuum region. Most recently, measurements of the multiplicity of the γ -cascades have been made at Berkeley by Tjøm *et al.*³⁾.

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γ -RAY MULTIPLICITIES FROM NUCLEAR REACTIONS: THEORY, INSTRUMENTATION AND ANALYSIS

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Expressions for interpreting γ -ray multiplicity measurements with a Ge(Li) and N NaI detectors are derived. A scattering chamber designed to allow measurements of multiple coincidences between a Ge(Li) detector and/or particle detectors and up to 12 NaI detectors is described. A special multiplicity recording unit has been constructed. It employs the strobed overlap coincidence technique and it is suitable for routing a multichannel analyzer or for interfacing to a computer. Details of the analysis procedure are discussed. The application of a computer code to multiplicity calculations is described.

1. Introduction

There are several classes of nuclear reactions in which knowledge of the number of emitted γ -rays is of importance. In heavy-ion induced reactions, for example, knowledge of the number of γ -rays from the emitted heavy-ion transfer products as well as their energy spectrum is important in understanding the de-excitation processes involved. In heavy-ion induced fusion reactions in the regions of deformed nuclei, knowledge of the first few moments of the distribution in the number of γ -rays can be used to infer the angular momentum distributions in the product nuclei or even in the compound system. Finally in light-ion induced fusion reactions describable by the compound statistical or by the hybrid model for nuclear reactions, measurements of the multiplicity of the γ -rays can help in understanding the mechanisms involved.

The simple fact that the coincidence rate in γ - γ coincidence experiments depends upon the number of γ -rays in cascade has only recently been used to deduce the cascade lengths. Multiplicity experiments that employed Ge(Li) and NaI detectors were reported by Tjøm et al.¹⁾ and Der Mateosian et al.²⁾ The former was a two detector experiment that involved a Ge(Li) detector for gating and a single NaI detector as a coincidence counter. Hagemann et al.³⁾ later employed a Ge(Li) and four NaI detectors. That arrangement allowed them to deduce not only the average multiplicity but also higher moments of the distribution as well.

In this paper we present in some detail the theory, instrumentation and method of analysis of experiments^{4,5)} employing as a strobing device a Ge(Li), a Ge(Li) coincident with several Si or time-of-flight neutron detectors, or a heavy-ion $\Delta E \times E$ detector telescope and a set of eight or nine NaI detectors. The theoretical expressions used to analyze the multiplicity measurements are derived. Two versatile multi-detector scattering chambers and their associated instrumentation are described. Finally, the computational procedure used in the analysis and the necessary corrections to the data are discussed.

2. Theory

In the following, the expressions necessary for the analysis of a multiplicity experiment employing a gating Ge(Li) detector and N NaI counters will be derived.

* Work supported by the U.S. Energy Research and Development Administration.

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(1974) 477

Nucl. Chem. **37** (1975) 1583

n. **36** (1974) 3639

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25 (1970) 1764

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Nucl. Sci. Eng. **50** (1973) 243

ain, K. I. Roulston, K. G. Standing,

A STUDY OF γ -DECAY MODES FROM HIGH ANGULAR MOMENTUM NUCLEAR STATES BY HIGH ORDER MULTIPLICITY MEASUREMENTS

O. ANDERSEN, R. BAUER, G. B. HAGEMANN, M. L. HALBERT^{*}, B. HERSKIND,
M. NEIMAN and H. OESCHLER^{††}

The Niels Bohr Institute, University of Copenhagen, Denmark

and

H. RYDE^{†††}

Roskilde University Center, Roskilde, Denmark

Received 17 May 1977

(Revised 23 August 1977)

Abstract: Reactions between ^{18}O and ^{16}O ions, with energies well above the Coulomb barrier, and $^{148,150}\text{Nd}$ target nuclei were used to study the cascades of γ -rays in the residual nuclei $^{161,162}\text{Er}$. A multi-counter setup was used in which the γ -rays were detected by a Ge(Li) counter coupled in coincidence with up to 9 NaI(Tl) counters. The γ -multiplicity has been studied for γ -rays cascading through states above and along the yrast line. Higher moments of the multiplicity distributions (shape parameters) were deduced from the data. These moments allow the construction of the spin distribution for the entry states. The difference between these spin distributions from reactions induced by ^{16}O and ^{18}O ions was studied. Also the fusion cross section was measured for the two systems as a function of bombarding energy and compared to the multiplicity results at two different excitation energies ($E_x = 49.5$ and 56.2 MeV). A subtraction technique has been applied in the data analysis with the intention to study the decay of a selected part of the high-spin region, and the results show: besides the $4n$ channel an unexpected large contribution of the $5n$ channel from this region. The sidefeeding patterns of the multiplicity distributions are extracted and show a significant difference for the ^{16}O and ^{18}O induced reactions, especially at the lowest bombarding energies. The results are compared with statistical model calculations.

NUCLEAR REACTIONS $^{148,150}\text{Nd}(^{16}\text{O}, \text{xn})$, $(^{18}\text{O}, \text{xn})$, $E = 69\text{--}78$ MeV; measured γ -ray multiplicities $\langle M \rangle$, and $\langle M \rangle^2$, variance and skewness; fusion cross sections σ_f , Enriched targets.

E

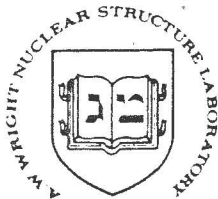
1. Introduction

The properties of nuclei at high angular momenta are presently being very much discussed. Intriguing problems concern the formation of nuclei in high-spin states

^{*} Exchange visitor from Oak Ridge National Laboratory, Oak Ridge, Tennessee, USA.

^{††} Present address: Centre de Recherches Nucléaires, Strasbourg, France.

^{†††} New address: Department of Physics, University of Lund, Sweden.



YALE UNIVERSITY

A. W. WRIGHT NUCLEAR STRUCTURE LABORATORY

P.O. Box 6666, 272 Whitney Avenue, New Haven, Connecticut 06511

22 JAN 90 10. 99

-R.L. GARWIN-

1.4

MEMORANDUM

January 17, 1990

TO: Steve Jones

FROM: Moshe Gai

SUBJECT: 1. Derivation of Moshe's efficiency formula (from report to DOE)
2. Comment by Dick Garwin

I have mailed to you the three papers where the most general case for hit probabilities are derived. If you work it out you will convince yourself that Moshe's formula from the interim report is a 1st order approximation of the most general solution, as I shall demonstrate here. The papers I listed are:

1. G.B. Hagemann et al.; Nucl. Phys. A245(1975)166.
2. O. Andersen et al.; Nucl. Phys. A295(1978)163.
3. L. Westerberg et al.; Nucl. Instr. Meth. 145(1977)295.

At first you will note that in Ref. 1 equations A.2 should be multiplied by the binomial coefficient $B(N,p)$ on the LHS and equ. A.3 should be multiplied by the same binomial on the RHS. This is correctly done in equ. 15 of Ref. 3. Please note that k of Ref. 3 is equal to $N-x$, of Ref. 1. This new notation was introduced in Ref. 3 so as not to sum binomial coefficients of value zero.

The most general solution was given by B.R. Mottelson (see p. 186 of Ref. 1), in equ. 14 of Ref. 1, which is the same equation as A.3. The hit probability is then:

$$P_{Np} = (-)^{N+p} \sum_{x=1}^N (-)^{x+1} * B(p, N-x) F_x * B(N, p)$$

In Ref. 1 we find: "To the first order in Ω^p , eq. (14) gives:"

$$= M * (M-1) * \dots * (M-p+1) \Omega^p * B(N, p)$$

You will note that since we don't run our array in coincidence with a gamma detector: $(M-1)$ of Ref. 1 $\rightarrow M$, as it appears here. But:

$$= M * N \Omega * (M-1) * (N-1) \Omega / 2 * \dots * (M-p+1) * (N-p+1) \Omega / p$$

Which is the so called Moshe's formula (actually was discovered by many nuclear physicist before me). I shall state again, there is nothing wrong with Moshe's formula as appeared in the report. This formula is clearly an approximation to the exact solution, which was appropriate for an interim report. As such it is

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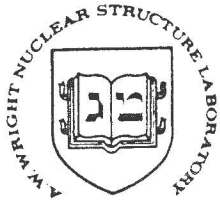
bound to break down (when the conditions for the approximation are not met). You will note that both "Moshe's formula" as appeared in our report, and equ. 15 of Ref. 1 yield efficiencies which are larger than one, for some value of M . i.e. at these values of M the approximation breaks down. A quick glance in Mottelson formula and Ref. 1 would have saved a lot of time.

2. Dick Garwin comment to me was of completely different nature, and I am afraid Steve you did not understand it. He correctly states, that if you have say a probability p of winning, if you do an experiment $1/p$ times that still does not mean that you will arrive at 100% winning probability, since the distribution is of Poisson type. This comment has nothing to do with the approximation introduced in Moshe's formula.

I hope that this finally settles these trivia!

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CONFIDENTIAL



YALE UNIVERSITY

A. W. WRIGHT NUCLEAR STRUCTURE LABORATORY

P.O. Box 6666, 272 Whitney Avenue, New Haven, Connecticut 06511

Dr. John Maddox
Editor, Nature
4 Little Essex St
London, WC2R 3LF
United Kingdom

January 10, 1990

Manuscript #: M06594R
H.O. Menlove et al.

Dear Dr. Maddox,

On October 19th, 1989, I offered my resignation as a referee on the above paper submitted by H.O. Menlove et al., a Los Alamos - Brigham Young University collaboration on "cold fusion". I cited a "conflict of interest" as the reason for my resignation, as I was at that time involved in a collaboration with Professor Steve E. Jones of BYU, to test the validity of the experimental results reported in the paper by Menlove et al. In retrospective my resignation, which at the time appeared to be the right thing to do, was a mistake. I now feel that in fact due to my collaboration with Steve E. Jones, I am in a better position for judging the quality of that paper and would like to offer you some additional information on this work.

I feel that throughout my dealing with the Los Alamos-BYU collaboration, as a referee, and in endless conversations and communications with Dr. Jones and Dr. Menlove, the collaboration has failed to address the specific criticisms raised about their paper and their experiment. It seems that they have neglected to reply, as I shall detail below, assuming that the criticism will just vanish. I am writing to you directly, because I fear the process involved runs deeply against one of the pillars of the scientific method: "the scientific debate", a process at which scientific criticism is directly addressed and properly dealt with.

As a referee I criticized the paper of Menlove et al. and pointed out shortcomings that could be addressed in simple new experiments. Instead of addressing these shortcomings with new data, Menlove et al. have apparently continued to search for "cold fusion" neutrons over the last half a year or so, in the same old way that they have been doing it, without altering their scientific method and without addressing my criticism.

Specifically, I pointed out the fact that the Menlove experiment has a large background, some factor of 20 to 2000 larger than any of the other "cold fusion" searches that were active at the time. I understood from Howard Menlove, that the experiment could be repeated in a quieter environment, in a nearby cave in Los Alamos laboratory, but this to my knowledge was not done. With such a large background I really don't know what they are measuring, and the bursts could arise from a lot of

possibilities; noise, MeVs electrons from cosmic shower that will activate the ^3He counter, neutrons from cosmic showers, etc. etc.

I noted that the control experiments were insufficient to show that a real effect was observed. In many searches for "cold fusion" neutrons, other researchers observed such bursts with the control sample, and much care is required to eliminate false detections. As you know in my study of the background of the Yale-BYU collaboration, a burst of neutrons from a control cell was observed, including some 20 neutrons. In our experiment the background is some factor of a 1000 times smaller than the Los Alamos-BYU one, but we still observe such burst events in the background. In this Los Alamos experiment, due to the large background, several additional control experiments must be carried out, for example with deuterium gas no Ti, or with various shielding to study the effect of a cosmic shower etc. etc., as detailed in my two reports to Nature from last summer. To my knowledge none of these control experiments were carried out. In my judgement the paper by Menlove does not provide convincing evidence for observing a real burst of neutrons and excluding background effects.

In the search for the random emission of neutrons, while the data were measured over one night, the background was measured over a different night, and simple fluctuation in barometric pressure between the two nights, could account for the 10% excess of neutrons above "background" reported by Menlove et al. The experiment performed here is incomplete in ruling out this possibility, even though a very simple experimental setup could be devised to address this issue correctly. As detailed in my report(s) the method used in the paper is in my opinion wrong on this aspect. I am surprised that after we had such a well publicized dialogue in Nature, back in May, lead by the comments of John Carpenter, on the effect of barometric pressure changes on cosmic neutron flux, the Los Alamos-BYU experiment still neglected this effect.

I have pointed out their over optimistic estimate of error bars, where they neglected systematical errors. The authors quote an error bar of the order of 0.5%, where I showed that merely the placement of the cylinder in the detector would cause a larger error bar. The authors to my understanding never in fact addressed this issue and continued to quote a statistical significance of their effect of some 10σ or so, which I have shown to be manifestly wrong.

In their manuscript they claim that bursts of neutrons are observed at a specific temperature of the cylinder, at -30°C , as the cylinder is brought to room temperature from liquid nitrogen temperature. This is in fact not supported by their data. Unfortunately, the collaboration does not have a detailed table including all the parameters of the bursts of neutrons that they observed. When I mentioned to Dr. Jones that in fact in the Menlove paper submitted to Nature, there are quite a few bursts shown to appear at room temperature, Dr. Jones in fact revealed that 40% of the bursts were obtained at room temperature. Dr. Jones does not see this as contradicting his statement, that the bursts are distributed mainly around -30°C , since the time they spent measuring at room

temperature is in fact much longer, so in his opinion they simply sampled the tail of the distribution over a longer time. Neglecting the logical structure of this argument, I have suggested to Dr. Jones a simple test of this idea by measuring only at -30°C , on top of the alleged distribution, and therefore increase the neutron yield. Dr. Jones replied that they did not do it since they claim that the cylinders have to soak in deuterium gas at room temperature for a long time, before neutron emission starts... This sounds to me like an ad-hoc theory on deuteriding, and I was unable to confirm it with any of my material science colleagues. Furthermore, this new "soaking procedure" is also contradicted by the data shown in Menlove et al, i.e. in Table III 50% of the bursts described appear before cycle 5 (within a few days) and all bursts appear before cycle 9 (slightly over a week). I should mention that many of these communications with Dr. Jones, were in writing and I would be happy to provide Nature with any of Dr. Jones' memoranda.

While collaborating with Dr. Jones the material science experts from Yale, reviewed the protocol developed in Los Alamos for treating the Ti samples. To our surprise we learned that the Ti samples were incorrectly handled in the Los Alamos experiment and without the removal of the surface oxide layer it is in fact possible that no deuterium have penetrated the Ti samples used in Los Alamos! A similar statement was made by the experts in Brookhaven, who studied deuteriding of Ti in the last 20 years as part of the 'hydrogen economy program'. In my talk at the "New York Material Science Society", on Nov. 2nd, this neglect in the Los Alamos experiment was met with a worry about the relevance of these results to the so called "cold fusion" phenomena. This fact escaped the criticism of my original referee report, due to insufficient details provided in the Menlove paper about the treatment of the Ti samples.

Unfortunately, the Los Alamos-BYU collaboration did not address this new criticism on the material science aspects of their experiment, by repeating the experiment with a treated surface, that should allow the formation of the deuteride phase. Even though one may imagine that this treatment of the surface would yield an increase in neutron emission. While one may claim that they still found an altogether new phenomena not related to "cold fusion", these problems should be clearly stated in the paper.

Instead of addressing the criticism on the treatment of the Ti, the authors have stated an ad-hoc theory that the deuterium is inserted to the metal through repeated cycling to liquid nitrogen temperatures, by cracking the surface. They claim that after the surface is cracked, the deuterium makes its way to the bulk of the metal over a prolonged period of time through the fresh cracks in the surface. Again, I was unable to verify this theory for "a new method" for deuteriding Ti, with any expert on material science. Furthermore, they all tell me that once the deuteriding process starts the metal is brought to saturation almost immediately, contrary to the claims of Jones et al.

All these arguments on the metal characterization could be addressed by simple measurements (i.e. pressure measurements) and by using standard procedures for material characterization. I am frustrated that after half a year or so of work, the laboratory at Los Alamos has neglected to characterize the Ti samples used in their work. We still have no quantitative evidence whatsoever, that the Ti samples in the Los Alamos work included deuterium in any part of the metal! I feel that this is an important issue that the authors should address.

As you know the Yale-BYU-BNL negative results, appearing in a joint report submitted on Oct. 27th to the DOE/ERAB panel on "cold fusion", are now almost five months old. I maintain that our data invalidate the Los Alamos-BYU results as appearing in the manuscript submitted to Nature. While I don't claim that one search should prohibit the publication of another, and again steering away from "conflict of interest", the result of our collaboration only emphasize the validity of the criticism against the Los Alamos-BYU paper.

It is important to mention the results of the Yale-BYU-BNL collaboration since I noticed an effort to delay the publication of our data and to down play our results. On Nov. 9th, 1989, Nature have published quotations from Dr. Jones on our collaboration which contradict scientific facts appearing in our report to DOE. Later Dr. Jones in a communication to Dr. Lindley and to myself, suggested that he was misquoted by Nature. Last Friday when I requested that Dr. Jones participates in a three way conference call including Dr. Lindley, Dr. Jones and myself, to resolve the matter, Dr. Jones retracted his previous statement that he was misquoted. But the fact still remains unchanged namely, a false criticism of the Yale-BYU-BNL experiment was published in Nature.

In the Nature comment Jones claims that the Yale-BYU-BNL experiment that lasted 10 days, was too short to allow the formation of the deuteriding phase. Again as mentioned before, the data in Table III of Menlove et al., contradict this statement (with 50% of the bursts occurring within a few days). While we have exactly repeated the Los Alamos procedures, including the length of the experiment and the number of cycles, as reported in the paper by Menlove et al. submitted to Nature, we find negative results with upper limits on neutron emission over an order of magnitude smaller than reported in Los Alamos, as quoted in our report to DOE. The sensitivity of the Yale experiment is much improved due to a smaller background with a similar detection efficiency.

It is my opinion, that the severe criticism of the Los Alamos-BYU experiment as outlined above (coupled with the negative results of the Yale-BYU-BNL experiment) invalidate the paper of Menlove et al. and the burden is now on the Los Alamos-BYU collaboration to prove that they performed a measurement of some relevance. This I feel, is certainly not

demonstrated in the manuscript submitted to Nature, and can only be addressed by performing the appropriate future experiments that will demonstrate their claims to be true.

Sincerely,

Moshe Gai

Moshe Gai
Associate Professor of Physics

cc: Dave Lindley
Phillip Ball.

New Energy Times Archive

(1.5)

 Received: by YALEVM (Mailer R2.03B) id 2225; Wed, 17 Jan 90 22:44:45 EST
 Date: Wed, 17 Jan 90 22:41:50 EST
 From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI@YALEVM>
 Subject: Hit probability
 To: Dick Garwin <rlg2@watson>

===== 76
 Date: Wed, 17 Jan 90 22:02:02 EST
 From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI@YALEVM>
 Subject: Moshe's Formula and Dick Garwin's comments
 To: Steve Jones <jonesse@byuvax>

MEMORANDUM

January 17, 1990

TO: Steve Jones

FROM: Moshe Gai

SUBJECT: 1. Derivation of Moshe's efficiency formula (from report to DOE)
 2. Comment by Dick Garwin

I have mailed to you the three papers where the most general case for hit probabilities are derived. If you work it out you will convince yourself that Moshe's formula from the interim report is a 1st order approximation of the most general solution, as I shall demonstrate here. The papers I listed are:

1. G.B. Hagemann et al.; Nucl. Phys. A245(1975)166.
2. O. Andersen et al.; Nucl. Phys. A295(1978)163.
3. L. Westerberg et al.; Nucl. Instr. Meth. 145(1977)295.

At first you will note that in Ref. 1 equations A.2 should be multiplied by the binomial coefficient $B(N,p)$ on the LHS and equ. A.3 should be multiplied by the same binomial on the RHS. This is correctly done in equ. 15 of Ref. 3. Please note that k of Ref. 3 is equal to $N-x$, of Ref. 1. This new notation was introduced in Ref. 3 so as not to sum binomial coefficients of value zero.

The most general solution was given by B.R. Mottelson (see p. 186 of Ref. 1), in equ. 14 of Ref. 1, which is the same equation as A.3. The hit probability is then:

$$PNp = (-)^{N+p} \sum (-)^{x+1} B(p, N-x) F_x * B(N,p)$$

In Ref. 1 we find: "To the first order in e^{**p} , eq. (14) gives:"

$$= M^{**} (M-1)^{**} \dots (M-p+1) e^{**p} * B(N,p)$$

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You will note that since we don't run our array in coincidence with a gamma detector: (M-1) of Ref. 1 is replaced by M, as it appears here. But:

$$=M*Ne*(M-1)*(N-1)e/2*...*(M-p+1)*(N-p+1)e/p$$

Which is the so called Moshe's formula (actually was discovered by many nuclear physicist before me). I shall state again, there is nothing wrong with Moshe's formula as appeared in the report. This formula is clearly an approximation to the exact solution, which was appropriate for an interim report. As such it is bound to break down (when the conditions for the approximation are not met).

Note that both "Moshe's formula" as appeared in our report, and equ. 15 of Ref. 1 yield efficiencies which are larger than one, for some value of M. i.e. at these values of M the approximation breaks down. A quick glance in Mottelson formula and Ref. 1 would have saved a lot of time.

2. Dick Garwin comment to me was of completely different nature, and I am afraid you did not understand it. He correctly states, that if you have say a probability p of winning, if you do an experiment 1/p times that still does not mean that you will arrive at 100% winning probability, since the distribution is of Poisson type. This comment has nothing to do with the approximation introduced in Moshe's formula.

I hope that this finally settles these trivia!

MOSHE GAI, (203)432 Steve Jones

1/17/90 Moshe's Formula and Dick Garw

Received: by YALEVM (Mailer R2.03B) id 2218; Wed, 17 Jan 90 22:39:22 EST
 Date: Wed, 17 Jan 90 22:36:18 EST
 From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI@YALEVM>
 Subject: Communication to Menlove
 To: Dick Garwin <rlg2@watson>

Dear Dick,

In continuation of our discussion on the phone and after I received a copy of the letter from John Schiffer, here is my reply to Menlove, for which I am awaiting response.

(Could you please confirm receipt of this message, since I am not sure which address I should use?) Moshe Gai.

Dr. H.O. Menlove
 Los Alamos National Laboratory
 "Nuclear Safeguard"
 Los Alamos, NM 87545

January 17, 1990

Dear Howard,

Please note that your letter to Dr. John Schiffer, dated December 18, 1989, a copy of which was forwarded to me, includes an error. While you stated

that the Yale experiment has an efficiency of 2%, it is in fact 28%. You attached an underlined statement, from the abstract of our interim report, that suggest that the efficiency is 2%. However, as we explain in details in the manuscript of the report, the data is analyzed in search of bursts in "the singles" mode, with 28% efficiency. Only in our forthcoming publication we use the time of flight coincidence mode, with 2% efficiency, to analyze the data in search of neutrons emitted in random fashion. I have explained this fact in my letter of December 21st, for which I have not yet received a reply. I forward a copy of this letter to Dr. Schiffer.

I should also mention again that the efficiency of your detector is not 11% as you state in your paper. A complete derivation of hit probabilities for a source of large multiplicity, was first derived by B.R. Motteslon. In our report we used a 1st order approximation of his equation, that you could use, or alternatively (when the approximation breaks down) you could write a small program that will evaluate the efficiency of your detector. I attach here a copy of a memorandum to Steve Jones on the subject.

I am looking forward for hearing your
comments on my letter of December 21st,

Sincerely,

Moshe Gai

cc: J.P. Schiffer
S.E. Jones

Dr. H.O. Menlove
Los Alamos National Laboratory
"Nuclear Safeguard"
Los Alamos, NM 87545

December 21, 1989

Dear Howard,

Thanks for your letter of December 12th, a reply to my letter of November 10th concerning some questions on your "cold fusion" data. Before correcting a few of the numbers as stated in your last letter, allow me to recount my questions concerning your method:

Since I am not an expert on deuteriding, I consulted the experts from material sciences. I shall quote here my colleagues (i.e. Jim Reily at Brookhaven):

1. In the absence of a treatment of the surface (i.e. your experiment) a

small layer of oxygen will in fact prohibit the formation of the deuteride phase. It is than possible that your samples did not have deuterium in the bulk. I would think that this is a very serious problem.

2. Your theory that one forms the deuteride by cracking the surface by repeated cycling over a prolonged period, was not familiar to any of the material science people I talked to. In fact even those who accepted this method as a possible new way of forming the deuteride told me that in their experience once a deuteride is formed the whole reaction goes very fast to yield a complete saturation of the metal. I don't know what is the origin of your statement that one needs to wait long time "...for diffusion of the deuterium into the lattice through the fresh (no oxide) surface".
3. Leaving all of these aside, in your manuscript I find in Table III, the description of 27 bursts, out of the 37 that Jones tell me you have. In all cases the sample showed activity prior or at cycle 5 (i.e. less than some 3-4 days). Some 11 bursts occurred within cycles 1-4, the Ti662 samples show burst before cycle 4. Where is the evidence that one needs to wait for a long time? In your paper on p. 6 I read: "A complete temperature cycle would take about one day for most cases. Some cycles were shorter, lasting 5h. A given cylinder will be put through 7 to 14 of these cycles. The neutron emission always ceased after a few cycles". If the neutron emission ceased after a few cycles why would you say that one needs to wait a long time?

Our approach was that since you claim that the neutron activity occurred around -30 degrees than one does not need to wait a long time and the cycle could be shortened, as we did in our experiment (our cycles are some 9 hours long). So in fact we exactly followed your procedure (as described in your paper). We had 12 cycles, with four cylinders, and using your data we should have observed quite a few bursts. Could you please let me know if you still stand by the paper you wrote, and why you claim that we run for too short a time. It is also worth mentioning that I run for twice as long as Steve Jones requested in his note to me on July 17, 1989, i.e. Jones requested about 5 days of running but we run for 10 days.

concerning your questions about our experiment:

The efficiency of our setup in burst mode is 28% (as quoted in our report to DOE) not 2% as you suggest. The 2% number you quoted is for time of flight data, that is not included in our report. The whole beauty of the system we have is that we can run it in any way we chose to. In the report we only included the analysis of the data for which the twelve detectors are used as counters with pulse shape discrimination (to identify a neutron) and pulse height data to include energy information. This is the simplest mode with which we can analyze our data, without all the bells and whistles that the system has. The time of flight data could be used to look for random emission, since in this case we have an extremely low background (of the order of 0.4 counts per hour) with a reasonable efficiency of 2%. The random emission is not discussed in our report even though we also have analyzed these data at Yale.

Our diagnostic of a burst is not complicated as you suggest and in fact very simple. For example in our report we write the formula for the efficiency of our system for different sizes of neutron source. You could use this formula to also determine the efficiency of your experiment which is not 11% as you stated. There is no cross talk in the system when you only use the detectors as counters (without time of flight information). Could you clarify what you mean by your statement on a possible cross talk?

In fact you should realize that a cross talk would only produce an artificial burst, which will make it harder to miss a burst. The events for which we see more than 2.45 MeV energy deposited in the detector, have a clear pulse shape corresponding to a neutron. Hence we know that we observed a neutron with too high energy (i.e. cosmic neutron). If the detector had both gamma and a neutron hit at the same time the pulse shape will register a gamma-ray not a neutron. So g-n events in the detector could not account for veto by the 2.45 MeV window, as you suggest.

Concerning your question of the self veto when gamma rays are included in the sample. We measured the self veto efficiency using a 252Cf neutron source. As you know this source have some 30 gammas for every neutron, so in fact yes our quoted self veto efficiency does include the possibility of having both gammas and neutrons emitted from the source.

Concerning the report on evidence from China. I would be very happy to see a publication if you have one. In the course of an experiment many events may take place, and it is only the final published material that counts. This material usually includes only those events for which the experimenter took all the care to convince themselves that they are looking at a real effect and are willing to put their name on it. I think I am going to stick to published results only.

Since your experiment is heavily criticized by material science people, for not treating the surface to allow for the formation of the hydride. Then why don't you do a new experiment with a treated surface? After all if you are right we may expect to see many more neutrons much faster when the deuteride phase is formed immediately, if I understand you correctly.

Along the same lines, I criticized your experiment for having a very large background (some 1400 counts per hour). Some time ago you have told me that you can repeat the experiment in a cave near your lab, in a quieter environment? Did you in fact do that? I feel that the criticism can only be addressed by doing new experiments.

I am afraid my conclusion is very different from yours. I don't think that we both can be right. Only history will tell us which one of us is wrong! I am looking forward for hearing from you and continuing the dialogue on these data.

Sincerely,

Moshe Gai

cc: S.E. Jones

(1,6)

Received: by YALEVM (Mailer R2.03B) id 4127; Tue, 23 Jan 90 18:00:06 EST
Date: Tue, 23 Jan 90 16:56:01 EST
From: "MOSHE GAI, (203)432 5195, FAX:(203)432 3522" <GAI@YALEVM>
Subject: Re: Counter efficiency.
To: RLG2@WATSON
In-Reply-To: Message of 23 January 1990, 09:57:10 EST from <RLG2@YKTVMV>

cc: S.E.Jones
K.G. Lynn (Brookhaven collaborator)

Dear Garwin,

At first let me say that I don't know what material was mailed to you by Jones. If it is the same thing that was mailed to me, than it is a serious misrepresentation of my work on calculating the efficiency of our array. Could I get a copy of what was mailed to you? You will recall that Menlove in his letter to Schiffer misquoted our report and in fact did not send a copy of my reply to his comments.

Concerning the hit probability. I believe I have a solution to the hit probability, that is in close form. I have incorporated this solution into draft number two of our paper which was mailed today by Federal Express to BYU and BNL. I will FAX to you those pages from draft 2, where the formalism is discussed, and Fig. 5 of our paper where all the hit probabilities are plotted as a function of the multiplicity for various hit folds. In this draft I also write the approximation that can be used, even though I do not use them in the paper. You would note that while the approximation and hit probabilities calculated using this approximation were listed in our interim DOE/ERAB report, they were never used as such to interpret the data.

The hit probability that I calculated as discussed in draft 2, does not include multiple scattering (one neutron hits two detectors), or double hits (two neutrons hit one detector). I understand that you set out to solve the hit problem including these facts. This is quite a formidable task, which I thought I solved as plotted in draft number 1 of our paper. Since then I realized that the problem is more complicated and decided to stick to the simpler case of no multiple scattering and no double hits. The multiple scattering and double hits will in fact increase our low folds hit probability, for a given multiplicity M. i.e. less neutron will correspond to a specific detected fold.

Concerning your assumption:

1. The efficiencies as listed by you are correct, as long as we both talk about a total efficiency (i.e. intrinsic efficiency times solid angle). Yes a neutron can multilpy scatter between detectors. This probability is not small, even though I neglected it in my draft number 2. You will note that this is not just a mathematical assumption, but in fact we measure in our experiment time of flight between a neutron hits in the central detector and a hit in a ring detector. As part of our data taking we use this double scattering for reducing the background. We measure a total efficiency of 2% for such double scattering events, from two central detectors to ten ring detectors. In my analysis of hit pattern I neglect such multiple scattering, even though they can yield an artificial burst. This was done to have as a conservative estimate as possible of limits on bursts.

2. Indeed calculating multiplicity from hit pattern (fold) is very hard especially when you have only a few events. Methods were developed to

012390..MG

calculate the various moments of multiplicity distributions (i.e. the mean, the variance, etc. etc.). The problem we are facing is different. Which is to quote a reasonably confident upper limit on the number of neutrons in a burst. My idea here was to look for the sum of unobserved hit probabilities. For example if we only observe one ring counter fire, then I sum the probabilities for folds=2, 3, ..., 10 ring counters, as a function of the multiplicity M. I quote the upper limit as that smallest value of M for which this sum yields 90% hit probability. Do you think this is correct?

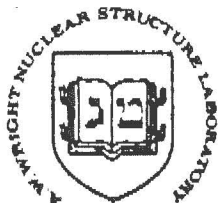
A few other comments. Since the efficiency for the central detector is much larger, the hit pattern for neutrons from the cells is dominated by the ten ring counters. This asymmetry was introduced partly to have a double scattering experiment, and partly to discriminate against background events for which all detectors have the same efficiency. In my analysis I only consider the hit probability of the array of ten ring counters.

Concerning the question of double hits. Since the spectrum of energy deposited by a neutron in our detector is equally distributed from zero to the neutron energy, the chance of a double hit summing to for example 4.4 MeV turns out to be 4% (= 0% at 5 MeV), based on energy consideration only. The pulse height of various detectors allow us to disentangle with some certainty a double hit from a high energy cosmic neutron.

Hope these random notes are of some help, and I shall FAX to you tomorrow the relevant pages from draft 2.

Best Regards and thanks for your help, Moshe Gai.

New Energy Times Archive



YALE UNIVERSITY

A. W. WRIGHT NUCLEAR STRUCTURE LABORATORY

P.O. Box 6666, 272 Whitney Avenue, New Haven, Connecticut 06511

DATE

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DR. Richard L. Garwin

FROM:

Moshe Gai

SPECIAL INSTRUCTIONS:

OUR FAX NUMBER IS: (203) 432-3522

For confirmation or questions, please call (203) 432-3090

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DRAFT #2

Yale 3074-1044

UPPER LIMITS ON EMISSION OF NEUTRONS FROM Ti IN PRESSURIZED D₂ GAS CELLS *

S.L. Rugari, R.H. France III, B.J. Lund, S.D. Smolen, Z. Zhao, and M. Gai
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Dept. of Physics and Applied Science, Brookhaven National Laboratory,
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K.W. Zilm
Dept. of Chemistry, Yale University, New Haven, CT 06511

ABSTRACT

We have used, over ten days (103 hours running time), a very low background detector to search for emission of neutrons from Ti in pressurized D₂ gas cells (cooled to liquid nitrogen temperatures). The cells contained some 200 grams of Ti and were very similar to that used in a recent Los Alamos - Brigham Young University collaboration. No statistically significant deviations from the background were observed for correlated neutrons emitted in bursts, or for neutrons emitted randomly. All events are shown (with 99% confidence) to be consistent with background. For bursts of neutrons (spanning up to 20 μ sec) we deduce (with 90% confidence) an upper limit on the bursts' size of 40. Our upper limit on the random emission of neutrons, of 0.008 n/sec (90% confidence), is a factor of 6 to 25 smaller than the evidence for random emission reported by the Los Alamos - BYU collaboration.

* Supported in part by USDOE contract number DE-AC02-76ER03074.

excess of the 2.45 MeV expected from a fusion event. We thus conclude (with 99% confidence, see above) that all high fold events are consistent with background events. It is worth noting that the detectors that register a large energy deposit from cosmic neutrons are placed at the less shielded front side of the set up, see Fig. 1.

Inspecting the time scale and energy deposited in detectors U5, U1 and U0 in the event of Run 45, listed in Table III, we may suggest that the event is consistent with one high energy ($E > 7.3$ MeV) cosmic-neutron (arriving from the less shielded front side, see Fig. 1), that undergoes triple scattering. It starts with one large angle scattering from detector U5 to its immediate neighbor detector U1. Then it travels its mean free path in liquid scintillator, of a few cm, in one nsec or so and then undergoes a small angle scattering in detector U1 toward the central detector U0. From U1 it travels (with a negative time of flight) for 17 nsec to the central detector U0. Note that a 2 MeV (or so) neutron travels, on the average, for some 15 nsec between a ring detector and a central detector. It appears that the parameters of the high fold event of Run 45, listed in Table III, are explained by this hypothesis of multiple scattering.

We have calculated the hit probability in our array using the formalism first developed by B.R. Mottelson and discussed in Refs. 4-6. The hit probability in our array of twelve detectors is determined by the 10 ring detectors. Therefore we only consider the hit probability in the ring counters. The hit probability $-P_K(M)$, of observing K detectors fired (K fold event), for a neutron source of Multiplicity M , is given by:

$$P_K(M) = (-)^{10+K} \binom{10}{K} \sum_{i=1}^{10} (-)^{i+1} \binom{K}{i} [1 - (1-i\epsilon)^M] \quad (2)$$

Where $\binom{N}{K}$ is the usual binomial coefficient (equal to zero for $K > N$), and $\epsilon=0.8\%$ is the efficiency of a single ring detector. Note that equ. (14) of Ref. 4, is missing the binomial coefficient $\binom{10}{K}$, which however is correctly inserted in equ. (15) of Ref. 6. It is easy to see [4], that to first order equ. (2) yields:

$$= M \cdot 10\epsilon * (M-1) * 9\epsilon * \dots * (M-K+1) * (10-K+1) \epsilon / K! \quad (3)$$

Care should be used in using the approximation, equation (3), as it is only valid for small probabilities (for large M, it is in fact larger than 1).

The total efficiency of the 10 ring detectors is given by $1 - \epsilon'$, where $\epsilon' = (1-10\epsilon)^M$, is the inefficiency of the array. Note that the array's inefficiency is equal to a fold $K=0$ event (i.e. $\epsilon' = P_{K=0}$). For example, for a multiplicity of 25 (35) the total efficiency of our ring counters is 88% (95%). These efficiencies are clearly more than adequate for our search of neutron bursts. We note that for a given event the sum of all hit probabilities for folds $K=1, \dots, 10$ must equal $1 - \epsilon'$, as one and only one event is detected, with an efficiency of $1 - \epsilon'$, (i.e. $\sum_{K=0}^{10} P_K(M) = 1$).

In Fig. 5 we show the hit probability of our array calculated using Mottelson's exact solution [4], equ. (2). For an event of multiplicity $M=100$, the array's total efficiency is 99.98%, and the most likely event is of fold $K=5$ or 6 (i.e. 7 or 8 neutrons detected in the entire array). No such events were observed in our experiment. We estimate for multiplicity $M > 40$, at least 90% hit probability for 2 or more ring detectors (i.e.

$P_2(22) + \dots + P_{10}(22) > 90\%$). Since no valid events are observed with more than one hit in the ring detectors, see Fig. 3c, we deduce (with 90% confidence) an upper limit on the size of neutron bursts of 40. Using the data of Menlove et al. [2], we expect some 2-4 such neutron bursts over the duration of our experiment.

In Fig. 6 we show the rate above background of neutrons emitted randomly. For these data the time of flight coincidence method was used [3], which yielded the very low background of 2 cph (0.4 cph in the upper hemisphere and 1.6 in the lower hemisphere). Using the backgrounds measured in Runs 40, 41, 51, 52, and 60, we deduce the neutron rates above background shown in Fig. 6, exhibiting no significant deviation from the background. The ensemble average of all our data yields the rate: -0.2 ± 0.4 cph above background. We then deduce (with 90% confidence), the random emission of neutrons not to exceed 0.6 cph, corresponding to a neutron source of 30 neutrons/hour (0.008 n/sec). This upper limit is a factor of 6 to 25 smaller than the rate reported (as 11σ) above background in Ref. 2.

IV CONCLUSIONS

We have searched for the emission of neutrons from "cold fusion", using an elaborate neutron detector system, and Ti samples as similar as possible to the ones used by Menlove et al. No statistically significant deviations from the background were observed, and upper limits on neutron emission, in both burst and random emission modes, significantly lower than reported by Menlove et al were obtained.

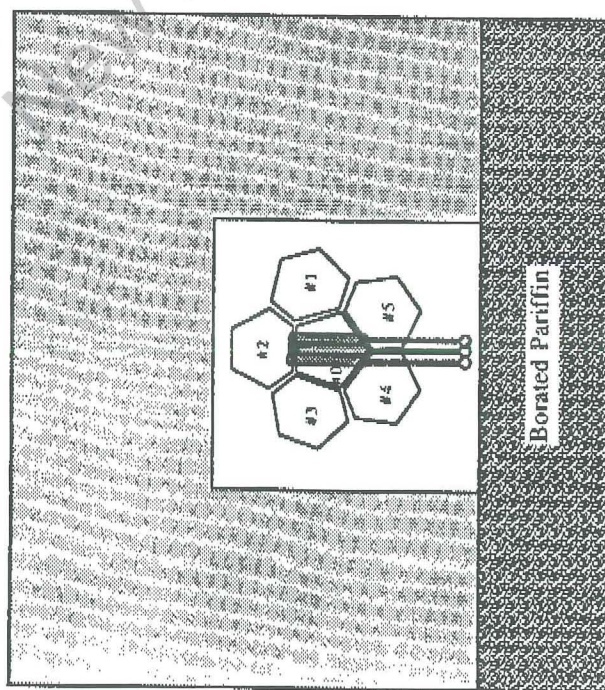
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1. S.E. Jones, E. Palmer, J. Czirr, D. Decker, G. Jensen, J. Thorne, S. Taylor, and J. Rafelski, Nature 338(1989)737.
2. H.O. Menlove, M.M. Fowler, E. Garcia, A. Mayer, M.C. Miller, R.R. Ryan, and S.E. Jones, to be published.
3. M. Gai, S.L. Rugari, R.H. France, B.J. Lund, Z.Zhao, A.J. Davenport, H.S. Isaacs, and K.G. Lynn, Nature 340(1989)29.
4. G.B. Hagemann et al., Nucl. Phys. A245(1975)166.
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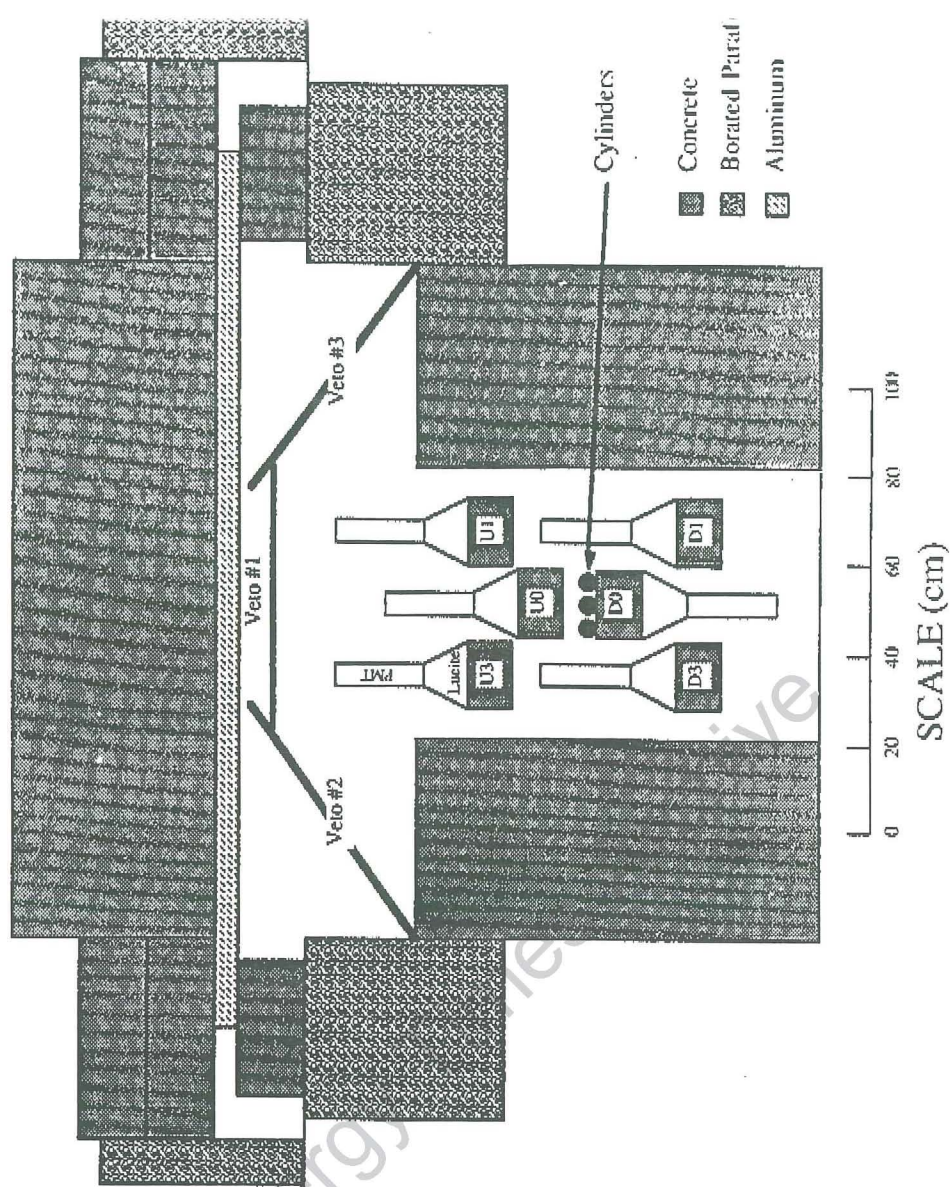
Fig. 1

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Top View



Front View



SCALE (cm)

SCALE (cm)

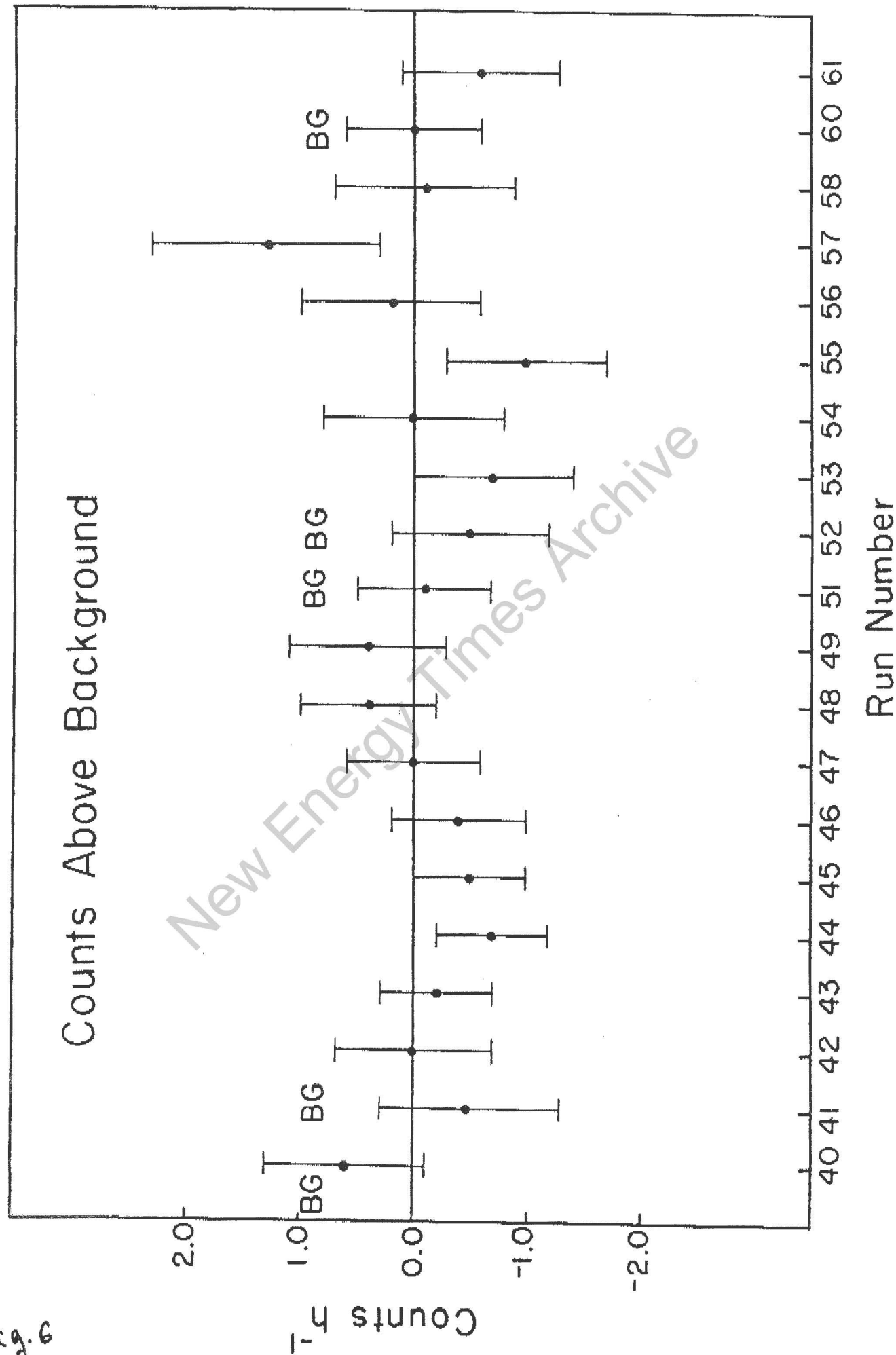


Fig. 6

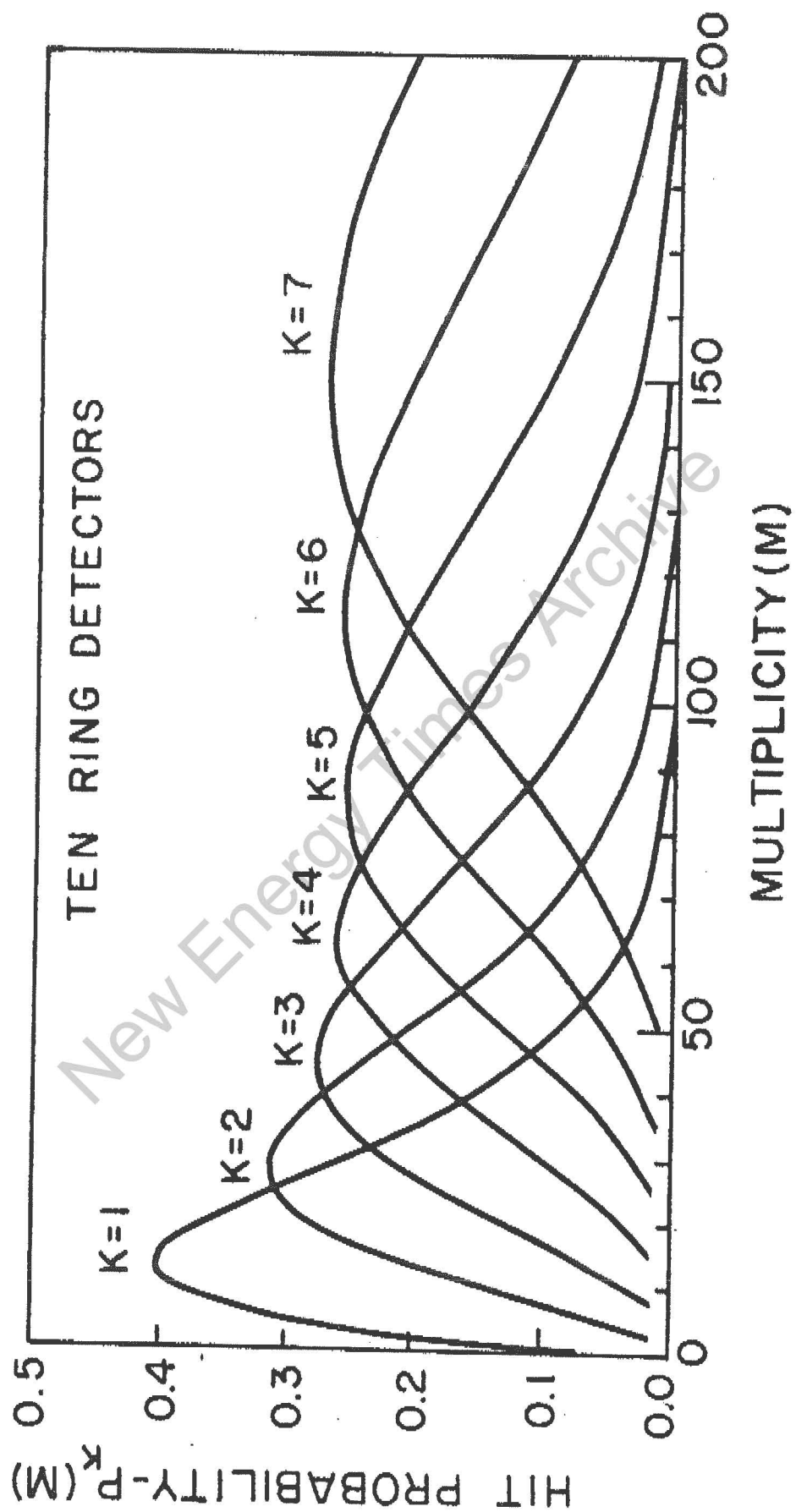


Fig. 5



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-R.L. GARWIN-

Date: 10-30-89

Physics Division
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Argonne, IL 60439-4843

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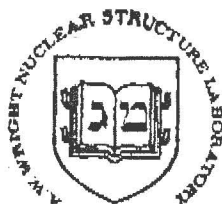
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Professor John Schiffer
Argonne National Lab
FAX: (312)972 3903
DOE/ERAB Committee

October 27, 1989
at 9:30 pm

Dear John,

Attach is our report on the Yale-BYU-BNL collaboration for release to the DOE/ERAB committee. I am sorry that the report is so much delayed, but we have received corrections from Dr. Steve Jones this evening with the last correction at 7:00 PM.

Sincerely,

Moshe Gai

Moshe Gai

P.S. my home number where I can be found over the weekend, is (203)488 9970.

Yale 3074-10.7

An Interim Report to DOE/ERAB Committee on "Cold Fusion", October 21, 1981**UPPER LIMITS ON NEUTRON BURSTS EMITTED FROM Tl IN PRESSURIZED D₂ GAS CELLS ***

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ABSTRACT

In a search for bursts of neutrons from Tl in pressurized D₂ gas cells ("dry cells"), no statistically significant deviations from the background were observed for events where five or more neutrons are detected over a ten day experiment, including 103 hours of counting with cells on, and 20 hours counting of various backgrounds. Up to four cells were used including some 60 grams (each) of 662-Tl fillings in a pressurized cylinder with 40-60 atmosphere of D₂ gas. Other Tl samples were used too. The samples were cooled to liquid nitrogen temperature and placed in front of the neutron detector while warming up to room temperature. Seven cooling cycles (of some 9 hours each) were used, for each sample. The neutron detector system included 12 liquid scintillator neutron detectors, arranged in a close packed geometry, with six detectors in the upper hemisphere and six in the lower hemisphere. A central detector placed 2 cm from the cells ($\epsilon=10\pm3\%$) was used, in each hemisphere, as a scatterer for a time of flight coincidence measurement, yielding the total coincidence efficiency of $\epsilon=2\pm1\%$. The system was also used in singles mode to allow for counting with large efficiency ($\epsilon=70\pm5\%$). Neutron data is characterized by measuring its pulse heights, pulse shapes, and in some cases its time of flight. Special attention was given to reducing the background by using massive shielding, cosmic ray veto counters and geometrical arrangement that allowed to distinguish between a background event and expected data events. The so obtained background rate are 100

cph in the "singles mode" and in the upper hemisphere 0.4 cph in the "coincidence mode". Two events including 5 detected neutrons were observed in the background runs (28 hours) and two events including 5 and 6 detected neutrons were measured with the cells on (103 hours). All four events were vetoed by the cosmic ray veto counter and they appear consistent with background events. The 3σ upper limit that we deduce on the emission of bursts in our data corresponds to a neutron source smaller than 27 neutrons over 103 hours from some 200 grams of Ti. We are currently continuing our data analysis in search for random emission and a detailed study of background effects that may reveal the origin of anomalous events.

* Supported in part by USDOE contract number DE-AC02-76ER03074.

DISCLAIMER

In a collaboration between Yale University, Brigham Young University and Brookhaven National Laboratory, data was taken at the Wright Laboratory at Yale, over the period spanning August 20-29, 1989. So far the data was analyzed independently at Yale University and Brookhaven National Laboratory, with an analysis at Brigham Young University in progress. In this report we release a preliminary set of our data from the above analyses, for the use of the DOE/ERAB committee. While this report is complete, we emphasize that the analysis of data is still in progress concerning other aspects of the data. The final results will be covered in our forthcoming article to be submitted for publication in the near future.

I. INTRODUCTION

Recent evidence for "cold fusion" [1] was reported by Menlove et al. in a Los Alamos - Brigham Young University experiment [2], where bursts of neutrons were reported from pressurized D₂ cells containing Ti fillings ("dry cells"). The cells were cooled to liquid nitrogen temperature and while warming up were placed in front of the Los Alamos neutron detection system including 12 ³He counters with total efficiency of $\epsilon \approx 30\%$. The bursts were observed around 30-40 minutes through the warm up cycle at a cell temperature of about -30°C. We have searched for bursts of neutrons using three to four such "dry cells" of essentially identical characteristics, and report here on results which do not show statistically significant deviations from the background, for high fold events. The detector system that we used can be operated in various modes and here we report on data using the high efficiency ($\epsilon \approx 28 \pm 5\%$) mode. Several other tests of "dry cells" fabricated in Brookhaven National Laboratory were also used in this search, with similar results obtained.

II. EXPERIMENTAL ARRANGEMENT

The "dry cells" used in this experiment included some 60 grams (each) of 662-Ti fillings, contained in a 40-60 atmospheres of D₂ gas. The cylinders were cooled to liquid nitrogen temperature and then allowed to warm up to room temperature. In Table I we list the experiments performed, and the cylinders used are specified in Table II, including details of the Ti material. In addition three cylinders prepared in Brookhaven National Laboratory were used and the details of these cells will be presented in our forthcoming publication. Each set of cylinders was studied over 7 warm up cycles of some 9 hours each, as listed in Table I.

The experimental setup is very similar to the one used in a previous Yale-Brookhaven collaboration on "cold fusion". We refer the reader to Ref. 3 for details of the neutron detectors and veto counters as well the operation of the system and the various cuts that can be placed on the pulse shape, pulse height and time of flight parameters in the analysis of the data.

In Fig. 1 we show the experimental arrangement which is used as scatterers and ton ring detectors, as described in Ref. 3. We list here the various improvements in the system as compared to that used in Ref. 3. (1) Twelve neutron detectors were used in this experiment, instead of the six used in Ref. 3. The detectors were arranged in two hemispheres allowing for measurement of up-down asymmetry. In this way one may discern downward moving (background) neutrons, from neutrons originating in the cells which move upward in the upper hemisphere. (2) Three veto counters were used to span a larger solid angle as shown in Fig. 1. (3) The shielding of the setup was improved with the use of some 20 tons of concrete blocks containing iron (of dimension 45x45x60 cm, each).

The efficiencies of our neutron detectors were measured using a calibrated ²⁵²Cf source, placed in the center of the detector system, as well as on the edge of the pentagonal central detectors. The efficiency of the central detectors was found to be $\epsilon(UO) \approx \epsilon(DO) = 10 \pm 3\%$ and each detector in

the outer rings, U1-U5 or D1-D5, was measured to have an efficiency of 0.8%. Thus when all detectors are counted without requiring a time of flight coincidences ("singles mode"), a total efficiency of $23 \pm 1\%$ is obtained. We emphasize that the efficiency of the central detectors is a factor of 12 larger than that of the ring detectors and for neutrons originating in the cells there is a very small probability that a ring counter will fire without the corresponding central detector. For background events, the efficiency of all detectors is essentially the same allowing a situation where the ring counters fire but not the appropriate central detector. In addition the efficiency for a neutron double hit in a ring detector is 0.006% and for a source including only 20 neutrons it is 0.2%. It is then very unlikely that in the data discussed below double hits occurred in any ring detector. Thus pulse height information from the ring detectors is very crucial for removing higher energy background neutrons. The time of flight coincidence efficiency of each pair of detectors, U01 and D01, was measured to be 0.2%, yielding a total coincidence efficiency of $2 \pm 1\%$ in the coincidence mode. The efficiency for vetoing cosmic ray related events was measured to be of the order of 65%. In addition we estimated the efficiency for detecting 2.45 MeV neutrons in the veto counters to be 0.6%. Thus for a neutron source including 20 neutrons it is of the order 12% and it is very unlikely that in any of the data discussed below the events were self vetoed.

We have measured for each detector its pulse shape, to facilitate neutron to gamma discrimination, its pulse height, for discrimination against background events where the energy deposited may be larger than 2.45 MeV expected from fusion events, and the time of flight between the counter (the central detector) and a ring detector. This allowed for studying the time ordering of events. While neutrons originating in the cells are moving upward in the upper hemisphere (i.e. central detector U0 fires before a ring detector U1-U5), the background events could be moving downward (i.e. with negative time of flight, see below). We however emphasize that it is possible in a burst including large number of neutrons spread in time, for one neutron to be detected in the central detector and a different neutron to be detected in a ring detector, in a time order that mimics a background neutron moving downward. Hence the time ordering of events have to be considered with care and may not be useful in all cases. The data was written onto magnetic tapes event by event (including up to some 50 parameters) for analysis using various cuts as discussed in Ref. 3. An event was accepted only when a signal was registered in one of the two central detectors U0 or D0. A gate 20 μsec wide, was used to allow for accepting all data falling within $\pm 10 \mu\text{sec}$ with respect to the trigger, i.e. before or after one of the two central detectors fired. Bursts of neutrons spread over more than 20 μsec would not be detected by our system. The detectors were calibrated using standard radioactive sources. The time of flight range of the TACs used was 10 μsec , with 8K dispersion allowing for the study of bursts spanning 10 μsec with an accuracy of 1.2% μsec . Time of flight for neutrons is measured starting from a central detector, hence in the case that a ring detector fired before its central detector the time of flight is negative. The time resolution of the system was measured for $\gamma\text{-}\gamma'$ events to be 2.5 nsec, as discussed in Ref. 3. The background rate in our experiment is 2 cph in the "coincidence mode" (with

$\epsilon=211\%$) and in the "singles mode" the background is 100 cph (with $\epsilon=2815\%$).

III. EXPERIMENTAL RESULTS

system in "singles mode", where the computer is triggered by one of the central detectors and all other detectors are inspected. In Fig. 2 we show a typical pulse shape gate on neutrons in detector U1. We have chosen a very broad gate on the pulse shape data (in one dimension), so as to make sure that no neutrons are excluded, but in the same time we include background gammas that fall within the broad gate. A histogram of number of detector fired (event fold) was constructed, as shown in Fig. 3, using such gates on the neutron pulse shape, as shown in Fig. 2. Only four events were observed to have high folds of five or six. Two were observed in two separate background runs (spanning 28 hours), in Runs 40 and 51 and two were observed in data runs (spanning 103 hours), in Runs 45 and 61. All 4 high folds events were vetoed by the cosmic ray veto counter. In Fig. 4 we show the time distribution of these high fold events. Note that the time calibration is 0.9 min per channel, and $t=0$ is a few minutes into the warmup cycle, after the cylinders are removed from the liquid nitrogen bath. The measured cylinder's temperature are shown as a function of time in Fig. 5. The most probable neutron multiplicity (M_n) can be calculated from an event of fold (K) using the efficiency for detecting an event of fold K given by:

$$\epsilon(K) = M_n \times \epsilon(12) \times (M_n - 1) \times \epsilon(11) \times \dots \times (M_n - K) \times \epsilon(12 - K)$$

For neutron source of multiplicity 125 we obtain 100% efficiency for detecting a neutron in each detector (fold=12), and for multiplicity of 20 we have 100% efficiency of detecting 5 neutrons. Hence a 5 (6) fold event most probably corresponds to a source with neutron multiplicity of $M_n = 20$ (24).

The list of all observed event parameters is given in Table III. Only in one event (Run 45) both U0 and D0 fired, as would be expected for neutrons originating in the cells. In all events the energy deposited in at least one ring detector is larger than the 2.45 MeV expected from a fusion event. The probability of these events being a hybrid event in which a neutron originating from the cell appears in chance coincidence with a cosmic background event is estimated to be of the order of 10^{-10} , and the probability for a double hit in a ring counter is estimated to be of the order of 0.2%, see below. We thus can conclude based on the energy deposited in the ring detectors that these events are consistent with background events. In fact inspecting the time scale and energy deposited in detectors U5, U1 and U0 we may conclude that the event of Run 45 include one neutron that does triple scattering including one large angle scattering from detector U5 to its neighbor detector U1, and then immediately followed by a small angle scattering into the central detector U0 (lying below the ring detectors (thus yielding to negative time of flight)). From our data we deduce an upper limit on neutron bursts from our cells to be smaller than 27 neutrons with 98% confidence (3σ).

REFERENCES

1. S.E. Jones, E. Palmer, J. Czirr, D. Decker, G. Jensen, J. Thorne, S. Taylor, and J. Rafelski, Nature 338(1989)737.
2. H.O. Menlove, M.M. Fowler, E. Garcia, A. Mayer, M.C. Miller, R.R. Ryan, and S.E. Jones, to be published.
3. M. Gai, S.L. Rugari, R.H. France, B.J. Lund, Z. Zhao, A.J. Davenport, H.S. Isaacs, and K.G. Lynn, Nature 340(1989)29.

FIGURE CAPTIONS

- Fig. 1: A schematic diagram of the experimental arrangement drawn to scale.
- Fig. 2: Typical two dimensional pulse height Vs pulse shape plot for neutron detector U1. In the analysis a one dimensional gate was used as shown in the projection along the pulse shape axis. Events listed in Table III as neutron or gamma events have pulse shapes that fall within 10 channels of the gate's limit (around channel 140).
- Fig. 3: Total number of high fold events observed in background runs (28 hours) and data runs (103 hours). The average event rates are proportional to the running time with the same proportionality factor for background and data runs.
- Fig. 4: Number of neutrons detected as a function of time into the warm up cycle. The time calibration is 0.9 min per channel, and $t=0$ is a few minutes after the cylinders were removed from the liquid nitrogen bath. In this surface plot, two adjacent events coincide to yield a continuous line.
- Fig. 5: Cylinder temperature as a function of time into the warm up cycle.

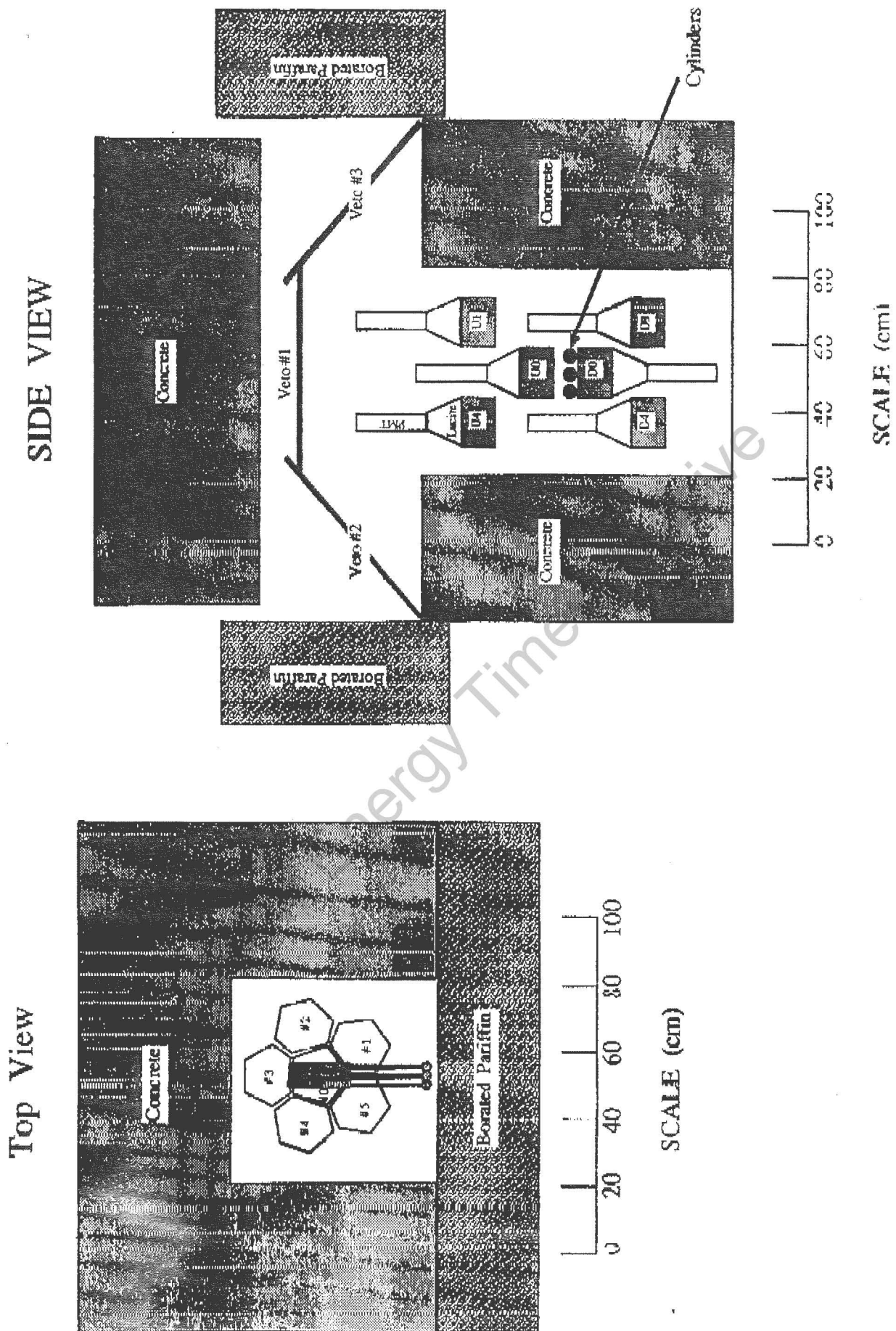


Fig. 1

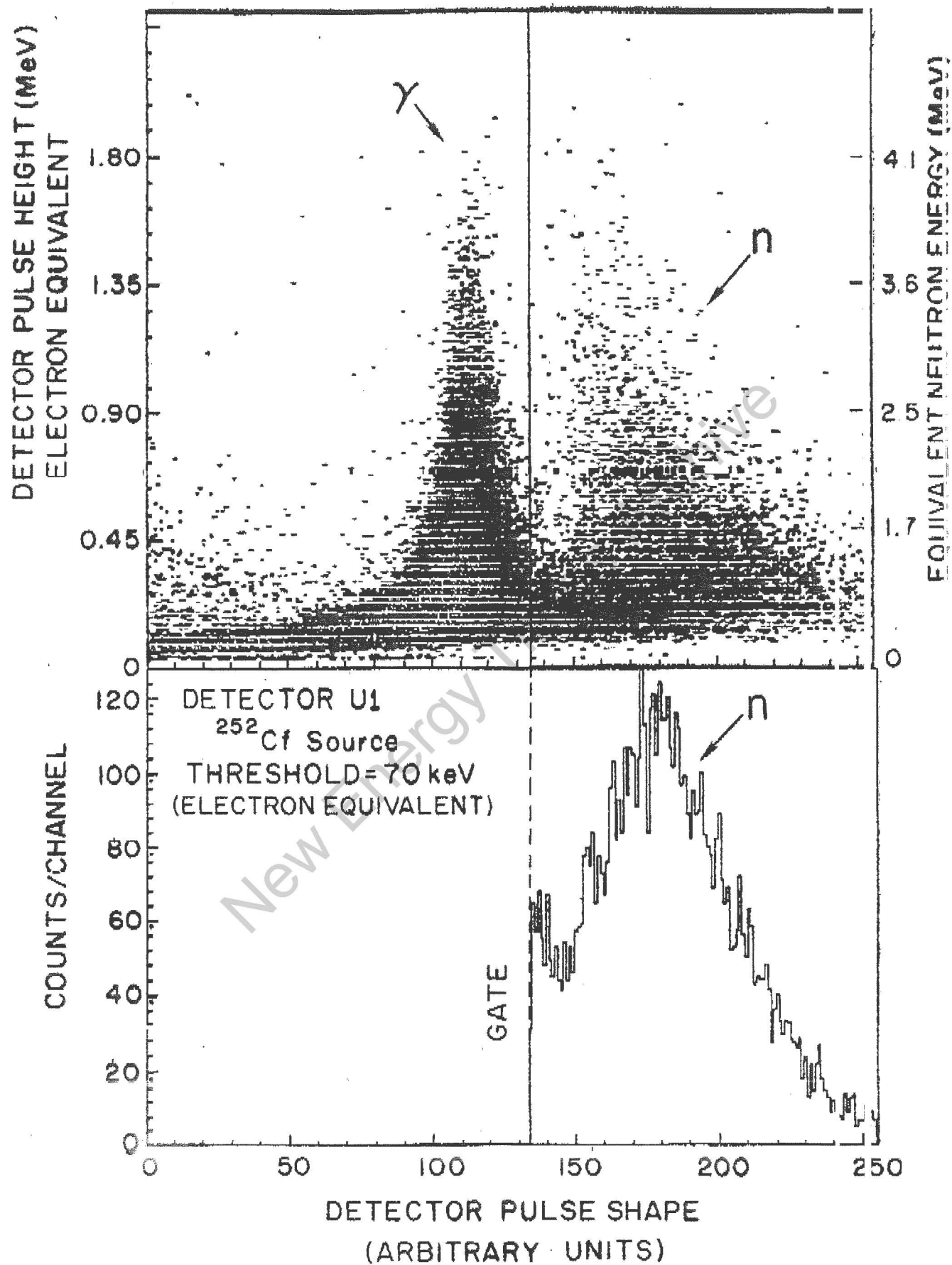
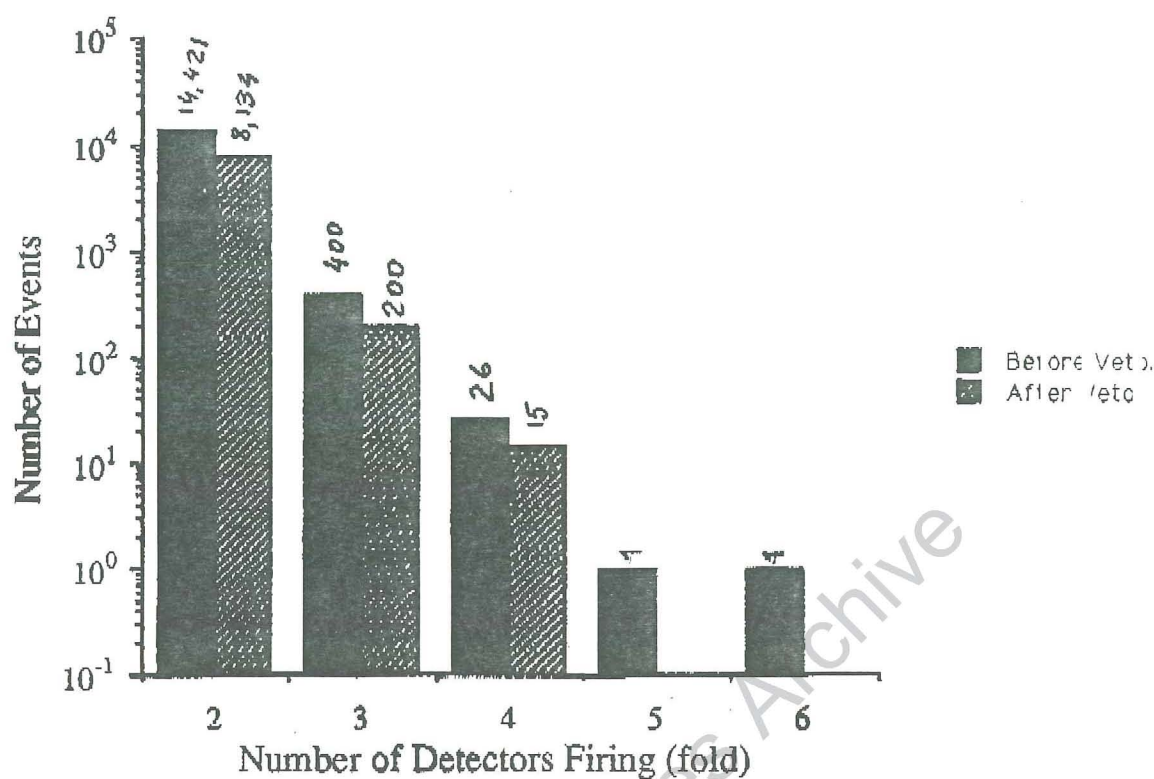


Fig. 2

High Fold Events From Data Runs (103 Hrs)



High Fold Events from Background Runs (28 Hrs)

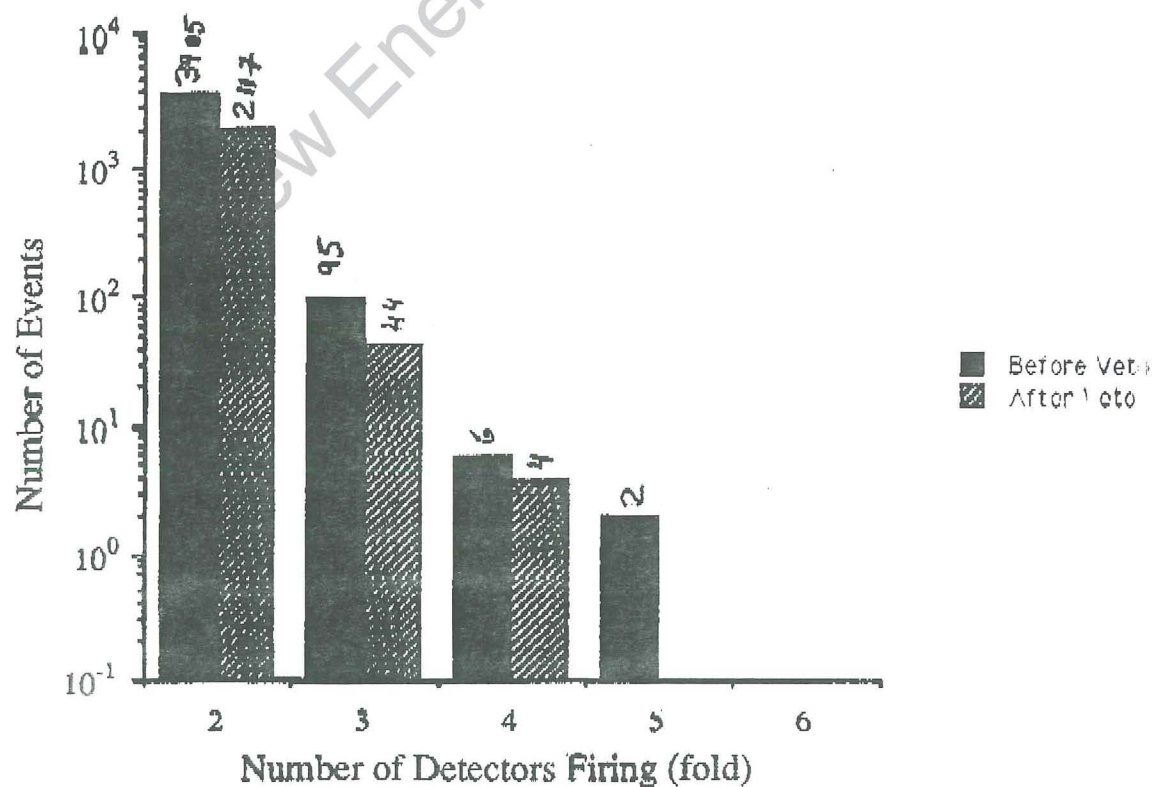


Fig. 3

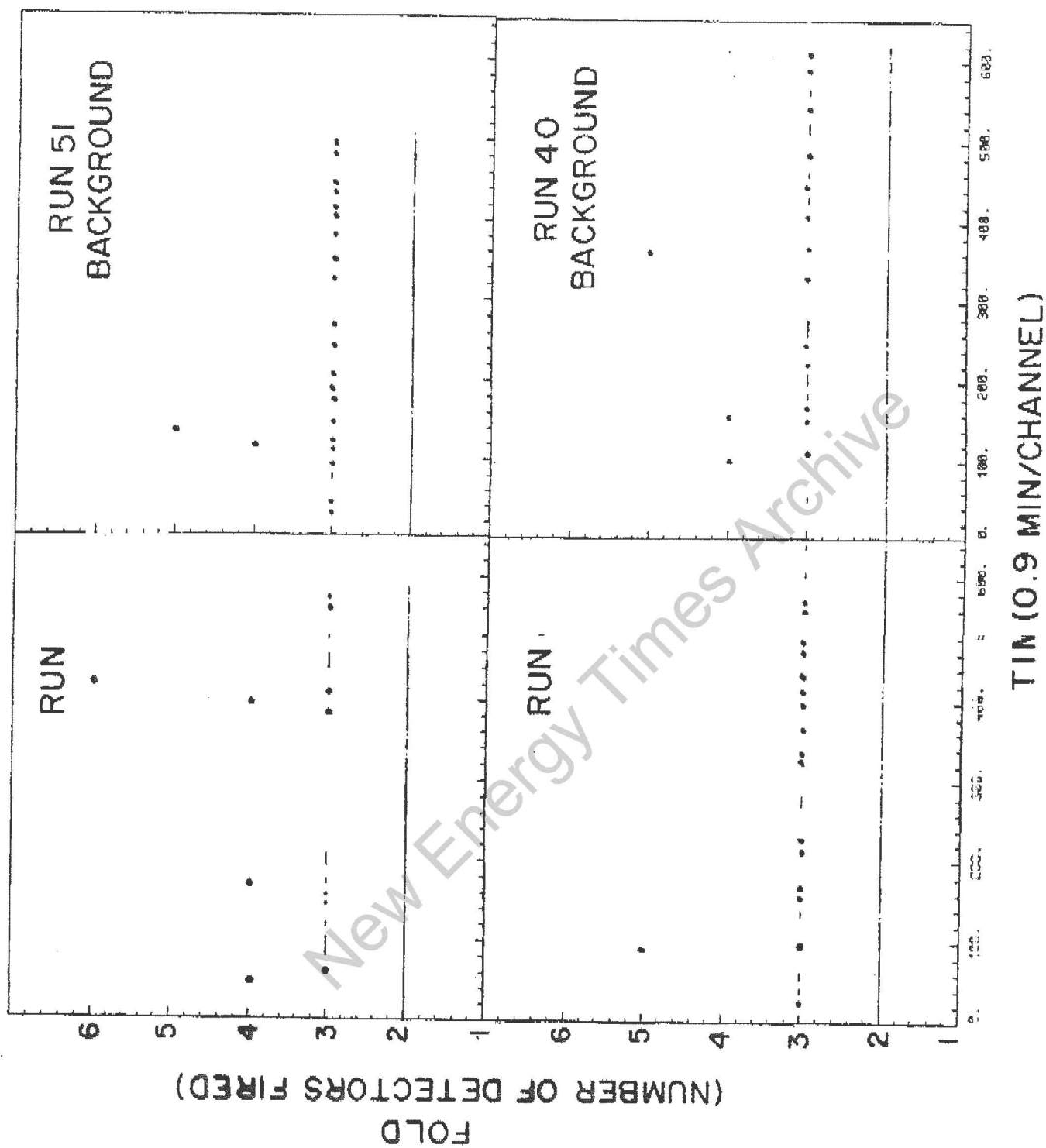


Fig. 4

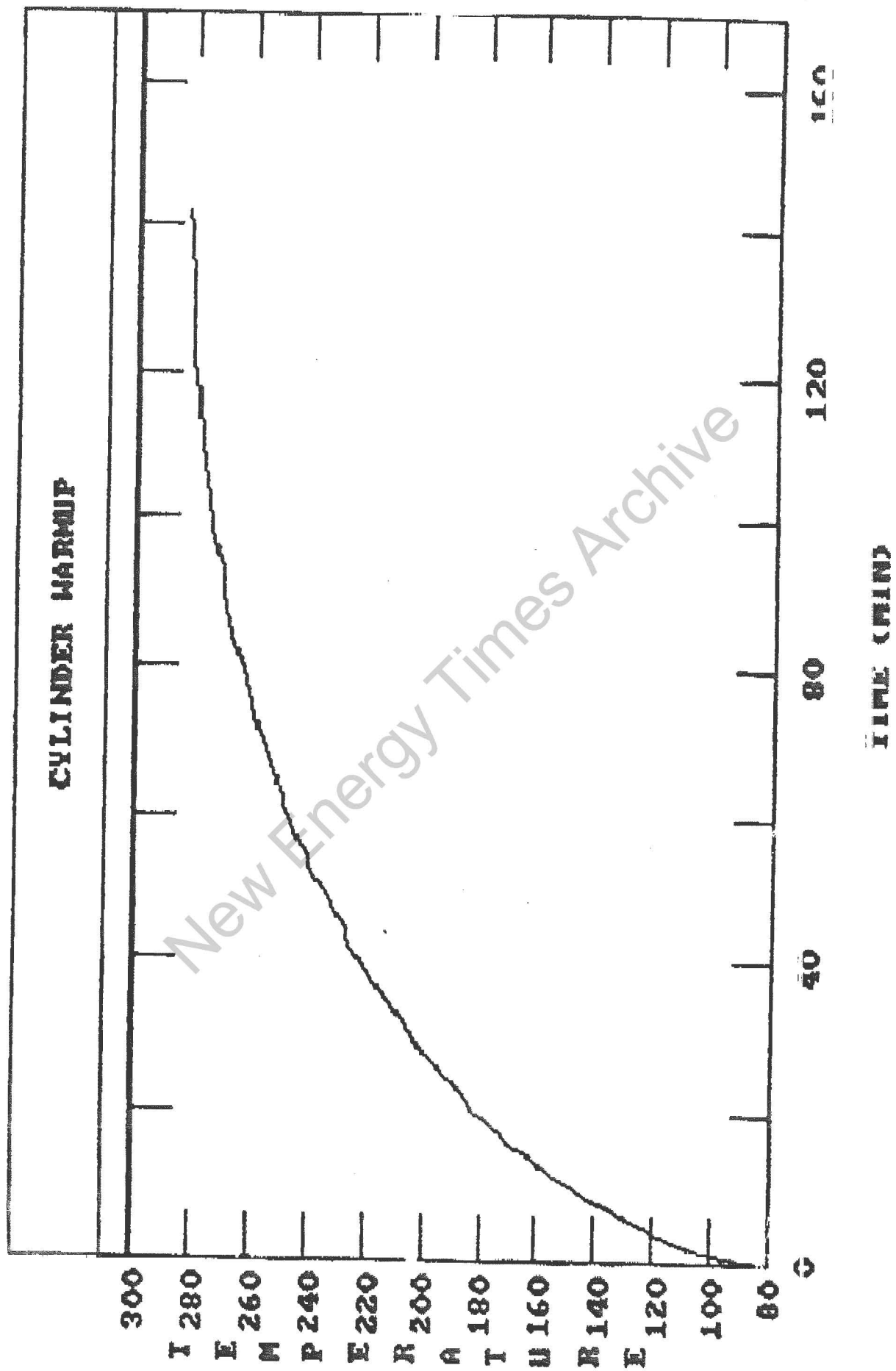


Fig. 5

Table I: Summary of Runs

Run Number	Type of Run	Cylinder Set	Length of Run	Blocks	Blocks Per Minute*
3	PuBe Neutron Source		0:03		
4	Cs 137 Source		0:01		
5	Co 60 Source		0:01		
11	Cf Source		0:40		
12	Cf Source		<7:48		
14	Cf Source		0:10		
20	Cf Source		0:07		
21	Cf Source		<0:10		
22	Cf Source		0:02	4812	2405
23	Signal Timing Calibration		0:19	1313	69.11
30	Bkgnd	None	0:02	8	4
31	Bkgnd	None	0:20	37	1.9
32	Bkgnd	None	8:16	873	1.75

At this point we adjusted on-line gates, pulse shape discriminators, and the voltages of detectors U0 and D0

33	Co 60 Source				
34	Cf Source		2:16	6732	49.3
35	Bkgnd	1/2 Lead Brick**	1:20	151	1.87
36	Bkgnd	1/2 Lead Brick**	0:23	54	2.35
40	Bkgnd	Set L	8:57	627	1.17
41	Bkgnd	Set L #1	3:33	255	1.20
42	Data Run	Set A #1,2,3	6:15	439	1.17
43	Data Run	Set A	12:30	860	1.15
44	Data Run	Set B	9:22	647	1.13
45	Data Run	Set C	9:46	651	1.12
46	Data Run	Set D	7:23	511	1.15
47	Data Run	Set D	7:53	544	1.15
48	Data Run	Set E	9:47	657	1.12
49	Data Run	Set E	6:26	439	1.14
50	Cf Source		2:57	8329	47.1
51	Bkgnd	Set F	7:30	509	1.15
52	Bkgnd	Set G	4:06	273	1.11
53	Data Run	Set H	4:03	279	1.15
54	Data Run	Set I	4:02	281	1.16
55	Data Run	Set K	4:01	279	1.16
56	Data Run	Set K	4:30	307	1.14
57	Data Run	Set K	4:00	277	1.15
58	Data Run	Set K	4:25	313	1.18
59	Cf Source		1:00	1797	30.6
60	Bkgnd	Set M	4:01	273	1.13
61	Data Run	Set J	8:44	585	1.12

Notes:

* This number is calculated after first subtracting off the seven header blocks from the blocks column.

** In runs 35 and 36, a 2"x 4"x 3.5" lead brick was placed between detectors U0 and D0.

Table II: Cylinder Sets Used in Experiments

Cylinder Set	Cyl #	Type of Gas	Type of Metal
Set A			
	#1	D ₂ @ 780 PSI	60g of Thick Ti662 chips from BYU
	#2	D ₂ @ 990 PSI	66g of Thin Ti662 chips from BYU
	#3	D ₂ @ 680 PSI	67g of Ti662 chips from Ormet
	#4	D ₂ @ 250 PSI	12g of Thin Ti662 chips from BYU and 4g of chips from Ormet
Set B			
	#1	D ₂ @ 720 PSI	60g of Thick Ti662 chips from BYU
	#2	D ₂ @ 980 PSI	66g of Thin Ti662 chips from BYU
	#3	D ₂ @ 680 PSI	67g of Ti662 chips from Ormet
	#4	D ₂ @ unknown PSI	12g of Thin Ti662 chips from BYU and 4g of chips from Ormet
Set C			
	#1	D ₂ @ 680 PSI	60g of Thick Ti662 chips from BYU
	#2	D ₂ @ 980 PSI	66g of Thin Ti662 chips from BYU
	#3	D ₂ @ 680 PSI	67g of Ti662 chips from Ormet
	#4	D ₂ @ unknown PSI	12g of Thin Ti662 chips from BYU and 4g of chips from Ormet
Set D			
	#1	D ₂ @ 660 PSI	60g of Thick Ti662 chips from BYU
	#2	D ₂ @ 980 PSI	66g of Thin Ti662 chips from BYU
	#3	D ₂ @ 680 PSI	67g of Ti662 chips from Ormet
	#4	D ₂ @ unknown PSI	12g of Thin Ti662 chips from BYU and 4g of chips from Ormet
Set E			
	#1	**	
	#2	D ₂ @ 980 PSI	66g of Thin Ti662 chips from BYU
	#3	D ₂ @ 680 PSI	67g of Ti662 chips from Ormet
	#4	D ₂ @ unknown PSI	12g of Thin Ti662 chips from BYU and 4g of chips from Ormet
Set F			
	#1	H ₂ @ 790 PSI	65g of Ti662 chips from LANL
	#2	D ₂ @ 790 PSI	No Chips
	#3	Vacuum	65g of Ti662 chips from LANL
Set G			
	#1	H ₂ @ 730 PSI	65g of Ti662 chips from LANL
	#2	D ₂ @ 780 PSI	No Chips
	#3	Vacuum	65g of Ti662 chips from LANL

Set H

#1	D ₂ @ 860 PSI	Ti662 chips from LANL
#2	D ₂ @ 860 PSI	Ti662 chips from LANL
#4	D ₂ @ unknown PSI	Thin Ti662 chips from Ormet*

Set I

#1	D ₂ @ 850 PSI	Ti662 chips from LANL
#2	D ₂ @ 850 PSI	Ti662 chips from LANL
#4	D ₂ @ unknown PSI	Thin Ti662 chips from Ormet*

Set J

#1	D ₂ @ 860 PSI	Ti662 chips from LANL
#2	D ₂ @ 860 PSI	Ti662 chips from LANL
#4	D ₂ @ unknown PSI	Thin Ti662 chips from Ormet*

Set K

#1	D ₂ @ 400 PSI	50g of FeTi
#2	D ₂ @ 414 PSI	45.9g of V(D ₂)
#3	D ₂ @ 415 PSI	20.3014g of Y metal
#4	D ₂ @ 200 PSI	24.98g of La(D ₂)

Set L

#1	H ₂ @ 780 PSI	Empty
#2	Vacuum	Empty
#3	Vacuum	Empty
#4	Vacuum	Empty

Set M

#1	Vacuum	50g of FeTi
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Notes:

- * This cylinder was smaller than the others and lacked a pressure gauge
 ** This cylinder had a set of used Pd electrodes. It started with D₂ gas at 100 PSI which rapidly fell to a level not measurable with the cylinder's pressure gauge do to the metal adsorbing the gas

Table III: Summary of High Fold Events

Detector	Energy Deposited (MeV)	Time of Flight (nsec)	Pulse Shape
Five-fold event in background Run 40:			
U0	2.8	0.0	n
U2	0.9	3.7	n
U4	0.9	3.7	n or γ
U5	4.4	-2.4	n or γ
D2	1.5	N/A	n
Five-fold event in background Run 51:			
U3	0.4	N/A	n or γ
U4	2.0	N/A	n
U5	4.4	N/A	n or γ
D0	0.9	0.0	n or γ
D1	1.5	3.7	n or γ
Five-fold event in data Run 45:			
U0	1.8	0.0	n
U1	1.1	-17	n
U3	0.6	3800	n
U5	4.4	-18	n
D0	3.3	N/A	n
Six-fold event in data Run 61:			
U1	0.6	N/A	n
U4	0.4	N/A	n
D0	5.4	N/A	n or γ
D2	1.6	740	n or γ
D4	1.1	1.2	n or γ
D5	4.2	1000	n

sent 01/29/90 by USPS Express
Mail

January 25, 1990

Dear Kelvin, Moshe, and Kurt, etc.,

Richard Garwin has looked over the "probability dispute" and produced hit probabilities that agree very well with Al Anderson's Monte Carlo predictions (see attached note by Al). I maintain therefore that Al's and my conclusions are valid regarding the faulty probabilities and associated misleading conclusions in the Interim Report of last October. (See reports by Al and I dated 3,6,15 January, and related memos).

I do feel obliged to reiterate my decision to discontinue collaborating with Moshe for the reasons I presented to you in my letter dated 15 January 1990. I see absolutely no reason to reconsider based on Dr. Garwin's work, and now reiterate one last time my firm decision. //

To those who may suggest this is due to some reluctance to publish a null result in a well-designed and well-executed experiment, I will ask that they study Al's and my January reports which show otherwise. And note that Al's analysis shows that double (ring) hit events in the foreground have a rate of $(0.25 \pm 0.08)/\text{hr}$ vs a background rate of $(0.1 \pm 0.05)/\text{hr}$ -- this result should not be ignored. (In speaking to Al yesterday about such events, he pointed out that the electronic inefficiency of each detector is strongly affected by how the neutron/gamma cuts are made. Determining the electronic inefficiencies per detector is a very difficult task because of the way the Yale electronics were set up, as shown by Al in his original analysis of the Yale data, but must be done before discussions of burst production or upper limits can be made quantitative. Al told me that until EACH detector efficiency is reliably evaluated by the Yale group, he will not undertake more analysis of the data -- this should really be a first step. The efficiency of each ring counter is NOT "0.008". Nor should 2+ bursts of 40 or more neutrons be expected in the Yale data based on Menlove as stated in Draft #2 -- this is grossly exaggerated and misleading. My analysis -- 3 Jan. report -- has been ignored, as has the short 20 μs gate in the Yale set-up relative to the 128 μs gate of Menlove.)

For the reasons given in my reports and Al's, I firmly disagree with the conclusions presented in the Interim Report and subsequent drafts. Although the probabilities in Draft #2 are finally getting close to Al's Monte Carlo predictions, the conclusions are virtually unchanged and do not reflect the analyses done by Al and myself. Let it be clear that my name must no longer appear on this report and that I do not intend to continue the dialogue for reasons stated in my 15 January letter (and other reports). //

Furthermore, I invoke what I consider a basic human right when I ask Moshe to cease phone calls which have all too frequently been rude and intimidating to me. I hope you understand.

As I stated several times beginning with my 3 Jan. report, I hope we can use what we have learned to do a truly definitive experiment. My invitation to you to participate (Moshe as an invited observer) is still extended.

I sincerely wish you all, all the best.

Steven E. Jones 1/25/90

Steven E. Jones

New Energy Times Archive

Response to notes from Dr. Garwin on Probability Calculations

A. N. Anderson

25 January, 1990

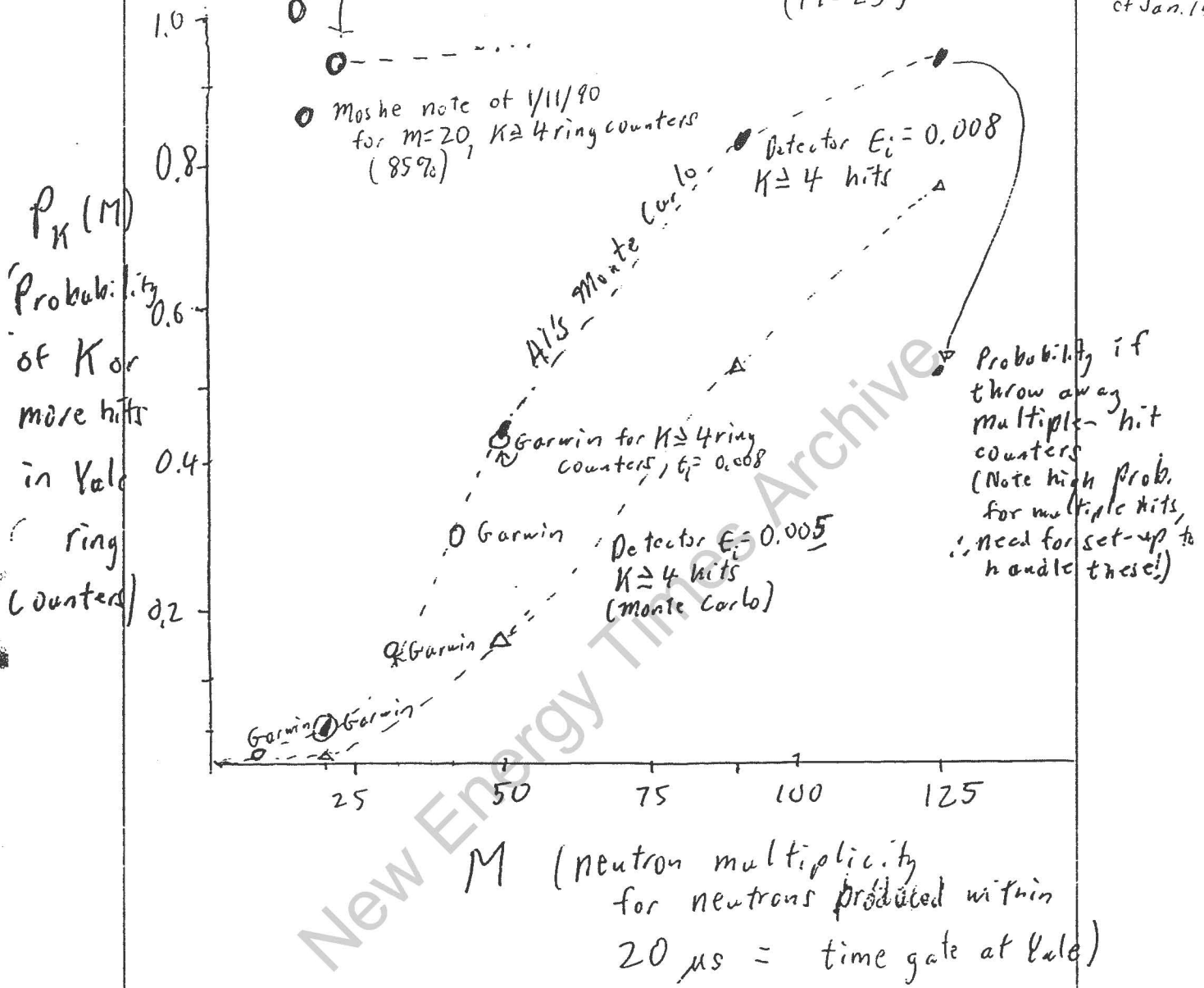
There were some minor differences between my calculations and Dr. Garwin's, in assumptions about the center counters, but the results are comparable for situations ignoring the center counters. Probabilities for 2-or-more and 4-or-more ring hits with bursts of 20 and 50 neutrons are presented below:

Neutrons	Hits	Garwin	Anderson
20	2	.4478	.4569
20	4	.0477	.0432
50	2	.8916	.9045
50	4	.4308	.4386

Each case assumes a ring counter efficiency of 0.008. The 'Anderson' values are taken from the 'ignore;any' column of Appendix B. of my original report. I feel this represents good agreement, especially considering that Dr. Garwin is using an algebraic expression and I was using a MonteCarlo. Considering the agreement thus obtained I see no reason to beat this particular horse further.

Jones 1/19/90

Interim Report to DOE/ERAB, for $K \geq 3$ ring counters (42 Central), $M=20$
 Moshe's claim: 92% for $K \geq 4$ (4 or more hits)
 ($M=23$) (Interim Report Draft #1)
 of Jan. 1990



Probability of 4 or more hits in Yale ring counters; contrasting Moshe's calculations and Anderson's Monte Carlo calculations. Note that if the Monte Carlo is accurate, then the Yale experiment was not set up well to detect bursts.